PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

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FOR THE YEAR MDCCXCVII.

PART I.

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ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March, 1752. And the grounds

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of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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Meteorological Journal kept at the Apartments of the Royal Society by Order of the President and Council.

THE PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged, for the year 1796, the Medal on Sir GODFREY COPLEY's Donation, to GEORGE ATWOOD, Esq. F. R. S. for his paper on the Construction and Analysis of geometrical Propositions, determining the Positions assumed by homogeneal Bodies which float freely, and at rest, on a fluid Surface; and also determining the Stability of Ships, and other floating Bodies.

PHILOSOPHICAL TRANSACTIONS.

I. *The* Croonian *Lecture. In which some of the morbid Actions of the straight Muscles and Cornea of the Eye are explained, and their Treatment considered. By* Everard Home, *Esq. F. R. S.*

Read November 17, 1796.

In two former Lectures, which I have had the honour of communicating to this learned Society, upon the subject of vision, I confined myself to the adjustment of the eye for seeing objects at different distances.

From the attention which in that investigation I necessarily paid to the natural actions of the muscles, and the structure of the cornea, I have been led to consider the effects which a diseased state of these parts will produce on the phænomena of vision. The observations I have made upon this subject, I now lay before this learned Society.

That I may be understood in giving an account of the diseases that arise from morbid actions of the straight muscles of the eye, it will be necessary to explain the effects which their natural actions are intended to produce; for these are

not confined to the separate, or combined actions of the muscles, but also vary according to the degrees of their contraction.

The first and most simple of these effects is that of moving the eyeballs in different directions.

The second is that of making the motions of the two eyes correspond with such a degree of accuracy, that when an object is viewed with both eyes, the impressions from the object shall be made on corresponding parts of the retina of each eye.

The third is that of compressing the eyeballs laterally, which renders the cornea more convex, and pushes forwards the crystalline lens, to adjust the eye to near distances.

Distinct vision with two eyes depends upon these different actions of the straight muscles; an imperfection in any one of them, as it renders the organ unfit to perform its functions, must be considered as a disease.

Three different diseases occur in practice, which appear to arise from morbid actions of the straight muscles. These are, an inability to see near objects distinctly; double vision; and squinting.

I shall consider each of these separately.

Of the inability to see near Objects distinctly.

As that action of the muscles which produces the adjustment of the eye to near objects, consists of the greatest degree of contraction usually exerted by them, it puts the fibres into a very uneasy state; which while in health they support with the utmost difficulty, and when affected by disease are unable to

sustain: under these last circumstances near objects cannot be seen at all without considerable pain, and never distinctly, the eye not remaining a sufficient time adjusted for that purpose. I cannot better explain the nature of this disease, than by giving an account of the symptoms which occurred in the following case.

A gentleman forty years of age, naturally short-sighted, of a delicate irritable habit from his infancy, never able to bear much bodily fatigue, being always soon tired by walking, or other exercises that required muscular exertion, had the following affection of his eyes.

His sight had been very perfect till he was nineteen years of age; at that time he resided in a part of the country where the ground consisted principally of white chalk, which produced an unpleasant glare; and his constant amusement both by day-light and candle-light was drawing, which he frequently pursued so far as to fatigue his eyes. While thus employed his complaints had their origin. The first symptoms were that of being unable to look long at any object without pain, and feeling uneasiness when exposed to strong light. The eyes to all appearance were free from disease, having no unusual redness, nor any purulent, or watery discharge. The plan that was first adopted for his relief consisted in lowering the system, both constitutionally and locally; but this treatment rendered him more irritable, and made his eyes rather worse than before; he therefore, after a trial of eight years, in different means of this kind, gave them entirely up. For the next five years, in which nothing was done to the eyes, the symptoms appeared to have been stationary; but at the end of that period, his mind suffering from an uncommon degree of anxiety, the

complaints in his eyes were evidently rendered worse; this effect, however, depended solely on the state of mind, for as soon as ever he recovered from his distress, the eyes also returned to their former state. In this condition I first saw him in the year 1795; and, at that time, his eyes had no external mark of disease, and were moved by the muscles in every direction without the smallest uneasiness. He could look at any thing that was at some distance, as the furniture in the room, the passing objects, &c. with perfect ease; but whenever he attempted to adjust the eyes to near objects, the effort gave so much pain, that although he succeeded in seeing them, he was almost immediately obliged to desist. Every attempt to write or read gave so much pain, that he became unable to do either; but as soon as the strain produced by such an effort was taken off, he was at ease. His disease therefore consisted in a want of power to adjust the eyes to near objects for a sufficient length of time to render them distinct, which of course incapacitated him from reading or writing. The cause of this disease appears to me to be a morbid affection of the straight muscles of the eyes, which allows them to perform all their intermediate contractions as usual, but not the extreme degrees of contraction without considerable pain.

As these symptoms have not, I believe, been before accounted for in this way, it may appear to many who have not seen similar affections of other muscles, that the present opinion is rather theoretical than practical; it will therefore be satisfactory to illustrate this disease in the muscles of the eye, by examples of the same kind of morbid action in other muscles, more within the reach of common observation. The following instances all refer to the muscles of the fore-arm and hand, employed in

actions with which every one is familiar, and show that these muscles are liable to be affected in the same manner as the muscles of the eye.

A gentleman, forty-six years of age, naturally of an irritable habit, which had been much increased by a long residence in the East Indies, was, about eight years ago, in a situation of great responsibility in that country. He was much engaged in writing, and previous to the sailing of a vessel for England, had, with a view to finish some dispatches of importance, written incessantly for a great many hours; the immediate effects of this exertion were simply fatigue, and stiffness in the muscles; but when he again attempted to employ the muscles in that action, he felt a nervous pain in the fore-arm, which was so severe as to oblige him to desist. This pain gave him considerable alarm, from the notion of its being of a paralytic nature, and many attempts were made to remove it. Recourse was had to electricity, and several other stimulating applications; but these always aggravated the symptoms, and they still continue. The circumstance in this case which is peculiarly applicable to my present purpose is, that the pain is only felt in the act of writing, the common motions of the fingers and thumb not giving the smallest uneasiness.

A gentleman about forty-six years of age, of a very irritable constitution, who had been in the habit of dealing cards for whole evenings together, was engaged in this employment one night for six hours; the weather was very warm, and he walked home in a state of perspiration, and went to bed. The window of his apartment, which faced the north, and was directly opposite to the foot of the bed, had been left open; the bed curtains were also undrawn. In the course of the night there was

a sudden change in the weather from hot to cold, and the wind having shifted to the north, blew directly upon his right arm, which was accidentally exposed. In the morning when he awoke his arm was in a very uneasy state. This however went off; but there was a pain in the muscles situated between the thumb and fore finger, and those of the fore-arm, which continued, and gave him great uneasiness. As it was supposed to be paralytic, blisters were applied to the origin of the nerves at the shoulder, and a visit to Bath was agreed upon as a necessary measure. The effects of the blister rather increased the complaint, which raised a doubt about its nature; and I found, upon a careful investigation, that particular muscles only were affected, which suggested an inquiry into the use that had been made of them. This inquiry led to a discovery of the real nature of the complaint, as only those muscles used in dealing cards were particularly affected. They were not in pain while at rest, but were unable to bear the least action without considerable uneasiness. This was greater at some times, than others; and although a year has now elapsed since the complaint came on, it is not entirely removed.

One of the principal tavern keepers in London was rendered very uneasy by a pain in the fore-arm, close to the elbow, which at times was very severe. Upon examining the parts, the pain was evidently not in the joint, but appeared to arise from an affection of the supinator brevis muscle, as the motion of that muscle gave pain. This I stated to him, but told him I was at a loss to find out in what way that part could have been injured; this was readily cleared up, when he informed me that the greatest pain he felt was in drawing claret corks, which he did with a jerk or sudden motion of the arm, and it was immediately after an

exertion of this kind that he had first felt the complaint. It was clear from this account that this particular muscle had been strained, and was rendered unfit to bear any violent action.

These cases will be sufficient to explain that a muscle, or set of muscles, may be unable to perform those actions which require the greatest exertion, although capable of performing all the others.

If then we consider the disease which causes the inability to see near objects as a strain upon the muscles, and compare it with the same disease in other muscles, there will be no difficulty in accounting for the bad effects produced by every thing that irritates, or weakens the parts themselves, or the general habit: it will follow, that such a mode of practice should be laid aside, and those means adopted by which the parts can be soothed in their sensations, and quieted and strengthened in their actions, since in that way only the muscular fibres can possibly recover their tone.

Of double Vision.

Many opinions have been advanced to account for the single appearance of objects when seen by both eyes.

Dr. Reid of Glasgow, who has taken much pains on this subject, has treated it with ingenuity and a great deal of knowledge; and the opinion he has advanced, of objects appearing single when the impressions from the object are made upon parts of the retina of the two eyes which correspond with each other, and double whenever that is not the case, is very strongly confirmed by the following observations upon double vision.

There are two circumstances under which double vision

takes place; one where the muscles of the eye do not correspond in their action, and therefore the two eyes do not bear equally upon the object; the other, where some change has taken place in the refracting media of one eye which prevents the pencils of light from impressing the corresponding parts of the retina of both eyes. Instances of double vision produced by these two modes have fallen under my notice.

It has been long ascertained by experiments, that when the eyes are not turned equally towards an object, it appears double, and the disease in the muscles which produces this effect is the subject which I now mean to consider. It will, at the same time, be proper to distinguish this kind of double vision from that which is produced by a change in the refracting media of the eye; and this will be best done by explaining the nature of those changes in consequence of which it occurs.

When one eye has had the crystalline lens extracted, the other remaining perfect, objects seen by both eyes will appear double.

This is a fact which was noticed in a former lecture, in treating of the adjustment of the eye. At first it appeared difficult to account for the double vision, particularly as the two images were entirely separate from each other. It could not arise from the absence of the lens, as that would not alter the situation of the images on the retina; and the two images being of different dimensions on similar parts of the retina, would appear to be one before the other. As the operation of extracting the lens in no respect affects the muscles of the eye, the action of the muscles would be the same as before, and therefore could not contribute to produce this effect.

The double vision in this instance appears to arise from the

cornea of the eye which had undergone the operation being rendered flatter than the other, and giving a different direction to the rays of light, so as to form an image on a part of the retina not corresponding with the part impressed in the other eye.

If the crystalline lens be extracted from both eyes, and the person applies a convex glass to one eye only, and looks at an object, it will appear double; but if the convex glass is moved in different directions before the cornea, there will be found one situation in which it makes the object single. In this instance the corneas and muscles of the two eyes are under exactly the same circumstances; and when the centre of the convex glass is directly in the axis of vision, the image on the retina of that eye is formed on parts that correspond with those impressed in the other; but whenever the centre of the convex glass is out of the axis of vision this does not take place, and the object appears double.

The experiments of which these observations are the result, were made upon the eyes of a lady who had lost the sight of both, by opacities in the crystalline lenses; but by submitting to have the lenses extracted recovered her sight, and had afterwards an uncommon degree of distinct vision; which made her a very favourable subject for experiments of this kind.

Having explained the two different modes by which double vision may take place in consequence of operations that render the refracting media of the eye imperfect, I shall now consider it when produced by a morbid action of the muscles.

Several cases of this kind have come within my own knowledge, and I am induced to dwell upon the subject, because some of them had been considered as arising from a defect in

the organ, and erroneously treated. The fact has been long established by philosophers that a defect in the muscles may produce such a disease, but as other causes may likewise do the same, I believe that such a defect has not been practically considered, as one of the diseases of the eye; certainly not as a very common one, which undoubtedly it will be found.

The first case of this kind which led me to pay attention to the subject, was that of a friend, a lieutenant colonel of engineers, who was in perfect health, shooting moor-game upon his own estate in Scotland. He was very much surprised towards the evening of a fatiguing day's sport, to find all at once that every thing appeared double; his gun, his horse, and the road, were all double. This appearance distressed him exceedingly, and he became alarmed lest he should not find his way home; in this, however, he succeeded by giving the reins to his horse.

After a night's rest the double vision was very much gone off; and in two or three days he went again to the moors, when his complaint returned in a more violent degree. He went to Edinburgh for the benefit of medical advice. The disease was referred to the eye itself, and treated accordingly; the head was shaved, blistered, and bled with leeches. He was put under a course of mercury, and kept upon a very spare diet. This plan was found to aggravate the symptoms; he therefore, after giving it a sufficient trial, returned home in despair, and shut himself up in his own house. He gradually left off all medicine, and lived as usual. His sight was during the whole time perfectly clear, and at the same time near objects appeared single; at three yards they became double, and by increasing the distance they separated further from each

other. When he looked at an object, it was perceived by a by-stander, that the two eyes were not equally directed to it. The complaint was most violent in the morning, and became better after dinner, when he had drank a few glasses of wine. It continued for nearly a twelvemonth, and gradually went off.

The above account of the disease was given to me by the patient himself, who is an intelligent man, very soon after his recovery. It was considered as a curious disease, and I had several conversations with Mr. RAMSDEN respecting it. The more we considered it, the more we were convinced that the disease had been entirely in the muscles; and this I explained to the patient at the time as my opinion.

It is now about eight years ago, and the gentleman has had no return of the disease; but for two or three years past has lost in a great measure the use of his lower extremities, being unable to walk alone.

Some time after the recovery of this gentleman, a house-painter, who had worked a good deal in white lead, was admitted a patient in St. George's Hospital, on account of a fever, attended with a violent headach. Upon recovering from the fever, he was very much distressed at seeing every thing double; and as the fever was entirely gone, he was put under my care for this affection of his eyes. Upon an inquiry into his complaints, I found them exactly to correspond with the case I have just described, and therefore treated them as arising entirely from an affection of the muscles. I bound up one eye, and left the other open; he now saw objects single, and very distinctly, but looking at them gave him pain in the eye, and brought on headach. This led me to believe that I had erro-

neously tied up the sound eye; the bandage was therefore removed to the other eye, and that which had been bound up was left open. He now saw objects without pain, or the smallest uneasiness. He was thus kept with one eye confined for a week, after which the bandage was laid aside; the disease proved to be entirely gone, nor did it return in the smallest degree while he remained in the hospital. Rest alone had been sufficient to allow the muscles to recover their strength, and thus produced a cure.

A repetition of cases, I am very sensible, is not the most pleasing mode of conveying information, except to medical men; I have therefore selected those only, which are absolutely necessary to explain the different phænomena of the diseased states of the eye at present under consideration. The cases brought forward with this view, are rather to be looked upon as the detail of so many experiments made in the investigation of the diseases, than as histories of particular patients.

When muscles are strained or over fatigued, to put them in an easy state, and confine them from motion, is the first object of attention; and this practice is no less applicable to the muscles of the eye, than to those of other parts.

Of Squinting.

Whenever the motions of the two eyes differ from one another, whether in a less degree, so as to produce double vision, or in a greater, turning one eye entirely from the object, the disease has been called squinting. What I mean at present to consider under this head is, where the deviation of one of the eyes from the axis of vision is greater than that by which ob-

jects are made to appear double; so that in this view, double vision is an intermediate state between single vision with both eyes, and squinting. Squinting has been very generally believed to arise entirely from an inability in the muscles to direct the eye properly to the object. There is, however, probably no original defect in the muscles; certainly none sufficient to sanction such an opinion; since the muscles of a squinting eye have the power of giving it any direction, but cannot do it without some degree of effort. The defect, therefore, appears to be principally in the eye itself, which is too imperfect to assist the other in producing distinct vision. From this imperfection, the muscles have not the same guide to direct them as those of the other eye; and, therefore, although perfectly formed, cannot make their actions exactly correspond with them.

In a squinting person, both eyes certainly do not see the object looked at. This is evident to a by-stander, who is able to determine, that the direction of one of the eyes differs so much from that of the other, that it is impossible for the rays of light from any object to fall upon the retinas of both; and, therefore, that one eye does not see the object.

The same thing may be proved in another way; for since a small deviation in the direction of either eye from the axis of vision, produces double vision, any greater deviation must have the same effect, only increasing the distance between the two images, till it becomes so great that one eye only is directed to the object. In squinting there is evidently a greater deviation from the axis of vision than in double vision, and the object does not appear double; it is therefore not seen by both eyes.

The circumstance of those who squint having an imperfect eye, is corroborated by all the well authenticated observations

which have been made upon persons who have a confirmed squint, which all agree in stating, that one of the eyes is too imperfect to see distinctly.

From these observations, it would be natural to suppose that the loss of sight in one eye, should produce the appearance of squinting, which is by no means the case; for when that happens, the motions of the two eyes continue to correspond, although not exactly; but the deviation is not equal to that which is met with in squinting; it is nearer to that which occurs in double vision.

The reason why the imperfect eye of a squinting person is directed from the object, while a blind one in its motions follows the other, is, probably, that the indistinct vision of the imperfect eye prevents the muscles from directing it to the object with the same accuracy as those of the other do; this small deviation from the axis of vision renders the object double, and interferes with the vision of the perfect eye; and it is in the effort to get rid of the confused image that the muscles acquire a habit of neglecting to use the imperfect eye. It may also happen, when the eye is so imperfect as not to receive a correct image of any object, that it may have been neglected from the beginning. Distinct vision being at once obtained by the perfect eye, the end is answered, and the mind is never afterwards led to employ the other.

The direction the eye takes under either of these circumstances is inwards, towards the nose, the adductor muscle being stronger, shorter, and its course more in a straight line, than any of the other muscles of the eye.

That the eye, when not accurately directed to the object, produces confused vision, and is for that reason turned away,

appears to be confirmed by the case of a patient, from whom I had extracted the crystalline lens. This man, at first, saw objects double, in a manner which extremely distressed him; but, after some months, acquired the habit of neglecting to employ the imperfect eye, and no longer found any inconvenience.

The different degrees of squinting appear to be in proportion to the imperfection in the vision of the eye, and, in some instances, the person is capable of seeing distant objects with both eyes, and only squints when looking at near ones. The following case is of this kind.

A young lady, twenty-three years of age, has been observed to squint from her infancy; this has not been considered by her friends as the consequence of any defect in her eyes, but as arising from the cradle in which she lay having been so situated, with respect to the light, as to attract her notice in one particular direction, so much as to occasion a cast in one eye. Her eyes are apparently both perfect; when she looks with attention at an object some yards distant she has no squint, but if her eyes are not engaged by any object, or a very near one, she squints to a considerable degree.

Upon being asked if she saw objects distinctly with both eyes, she said certainly, but that one was stronger than the other. To ascertain the truth of this, I covered the strong eye and gave her a book to read; to her astonishment, she found she could not distinguish a letter, or any other near object. More distant objects she could see, but not distinctly. When she looked at a bunch of small keys in the door of a bookcase, about twelve feet from her, she could see the bunch of keys, but could not tell how many there were.

To see how far the two eyes had the same focus, she was desired to look at an object in the field of a microscope, and it was found that she saw most distinctly with both eyes at the same focal distance, although the object was considerably more distinct to the perfect eye than to the other; so that the focuses of the two eyes were the same.

I desired her to cover the perfect eye, and endeavour to acquire an adjustment of the other to near objects, by practising the use of that alone. At first she was unable to see at all with the imperfect eye, but in some weeks she has improved so much as to be able to work at her needle with it; this she cannot do long at any one time, the eye being soon fatigued and requiring rest, though without giving pain. She is unable to read with the imperfect eye. These trials have only been made in the course of two months, for a few hours in the day, and her friends think that she squints less frequently than she did.

In this case it is probable that the imperfect eye never had acquired the power of adjustment to near objects; for as distinct vision seems necessary to direct the muscles in their actions, the perfect eye would require less practice to adjust itself than the other; and as soon as the near object became distinct to one eye, no information being conveyed to the mind of the failure in the other, all efforts to render its adjustment perfect would be at an end, and it would ever after be neglected, while the perfect eye was in use.

Squinting, according to these observations, appears to arise from the vision in one eye being obscure. It may, however, be acquired in degree by children who have the lenses of their eyes of different focuses; or have one eye less perfect in its

vision than the other, living constantly with those who do squint, and, by imitation, acquiring a habit of neglecting to use one eye.

The power of squinting voluntarily may also be acquired at any age. This we find to be true in persons who look much through telescopes; they are led to apply the mind entirely to one eye, not seeing at all with the other. In this case the neglected eye will at first, from habit, follow the other; but in time, if frequently neglected, may lose this restraint, and be moved in another direction. Some astronomers, whose eyes have been much used in this way, are said to be able to squint at pleasure.

From this view of squinting, it takes place under the three following circumstances: where one eye has only an indistinct vision; where both eyes are capable of seeing objects, but the one less perfect in itself than the other; and where the muscles of one eye have acquired from practice a power of moving it independently of the other.

Where squinting arises from an absolute imperfection in the eye there can be no cure.

Where it arises from weakness only in the sight of one eye, it may, in some instances, be got the better of but to effect the cure there is only one mode, which is that of confining the person to the use of the weak eye by covering the other; in this way the muscles, from constant use, will become perfect in the habit of directing the eye upon the object, gain strength in that action, and acquire a power of adjusting the eye; when these are established in a sufficient degree, the other eye may be set at liberty. The time that will be necessary for the cure must depend upon the degree of weakness of the sight, and

the length of time the muscles have been left to themselves; for it is with difficulty they acquire an increased degree of action after having been long habituated to a more limited contraction.

Of the Nature of the Cornea, some of its Diseases, and Mode of Treatment.

The cornea of the eye, as the name implies, has been considered of a cuticular nature. Baron HALLER compares it to the nails in a soft state, and believes that in its regeneration it resembles the epidermis.

This opinion is founded upon its want of sensibility, and having no vessels which carry red blood; the appearance it puts on when preserved in spirits, which is exactly similar to the nails at their roots, probably confirmed this supposition.

As the cuticle is devoid of life, it is only under the influence of disease during its growth; once formed, it continues unchanged. The cornea, were it of the same nature, would be equally incapable of taking on new actions from disease, or any other cause; but we find, on the contrary, that it undergoes many changes, which exactly correspond with those which the living parts of an animal body go through when under the influence of disease, from which I am induced to consider it alive; and I find that many of the present teachers of anatomy are of the same opinion.

To prove that the cornea has life it is necessary, as a previous step, to shew, that being supplied with vessels which carry red blood, and having sensibility, are not essential to the possession of the living principle; for this purpose all that is required is to

demonstrate that there are living parts which have neither the one nor the other. Tendons and ligaments in a natural state are instances of this kind. That these parts are not supplied with red blood is obvious to the eye of a common observer; no illustration will therefore be required to substantiate that proof. That they are not endowed with sensibility was, I believe, first taught by the late Dr. WILLIAM HUNTER,* who published the following account of it.†

In a case where the last joint of the ring-finger had been torn off, half an inch of the tendon of the flexor muscle projected beyond the stump; this it was thought right to remove; and to ascertain whether it was possessed of sensibility, the following experiment was made: a piece of cord the thickness of the tendon was passed round the wrist and along the side of the finger, so as to project even with the end of the tendon; the man was then told to turn away his head, and tell which of the two were cut through; the tendon was divided, and the man declared it was the string, not having felt the smallest degree of pain.

This proof is satisfactory; but that the cornea is possessed of life, by no means rests upon any negative proofs; which I shall now endeavour to explain.

The cornea in its structure is made up of membranous laminæ. One of these appears to be a portion of the tunica conjunctiva, but it is either so extremely thin, or so intimately connected with the lamina next to it, as not to admit of more than a very partial separation from it; another lamina, as I

* This doctrine was first taught by Dr. HUNTER, in the year 1746. HALLER made experiments proving the same thing in 1750.

† Medical Observ. and Inquir. Vol. IV. page 343.

have shewn in a former lecture, is a continuation of the tendons of the four straight muscles; but as both these laminæ have the same properties as the other parts of the cornea, and are not to be distinguished from them, they must be considered in every respect as a part of it.

The tunica conjunctiva and tendons, a continuation of which forms these anterior laminæ of the cornea, are allowed to be living parts, and the portions that make part of the cornea are not to be distinguished by their structure from the rest; we must therefore suppose them to be also composed of living parts.

When the cornea is wounded it unites, like other living parts, by the first intention. If the wound is made by a clean cutting instrument the cicatrix is small; but if by a blunt instrument it is larger, extending further into the neighbouring parts of the cornea, and a greater quantity of the coagulating lymph of the blood being required to procure the union.

Although the cornea, when divided in the operation for extracting the crystalline lens, commonly unites by the first intention, this union is in some cases attended with inflammation, which produces an opacity of the cornea; in other cases the inflammation exceeds the limits of adhesion, and the whole internal cavity of the eye proceeds to a state of suppuration. These stages of inflammation are only met with in parts possessed of life.

It is true, that an injury may be committed to the cornea, such as a small piece of metal sticking in it, which from the indolent nature of its substance, shall remain there for months without producing inflammation; but an irritation of a less violent kind upon the edge of the cornea, by which the tunica conjunctiva is also affected, will produce inflammation upon

that vascular membrane, which may extend itself upon the cornea; for it is impossible that the vessels of the cornea, which naturally carry only lymph or serum, can be made to carry red blood, unless the irritation extends to some neighbouring part supplied with red blood.

That vessels carrying red blood have been met with upon the cornea in a diseased state, is doubted by HALLER; he does not altogether deny it, but the assertion, he says, requires proof, as he is not satisfied with the authorities of PETIT and others whom he quotes upon that subject.

It is so common a thing in inflammations of the eye to have the branches of the arteries of the tunica conjunctiva continued upon the cornea, that every practical surgeon must have met with it. In some instances of this kind, which have come immediately under my own care, I have examined these vessels with a magnifying glass, and have seen distinctly small arteries from the tunica conjunctiva, uniting upon the cornea into a common trunk larger than any of the branches that supplied it, and this trunk has sent off other branches distributed over the cornea.

These vessels may, by some physiologists, be supposed to be continued upon the lamina of the tunica conjunctiva, which is spread over the cornea; this, however, is not the case, as they pass behind it, and therefore belong as much to the lamina under them as that which is over them; and, in many instances of disease, vessels carrying red blood are met with in the substance of the cornea still deeper seated. This has been seen by Professor RICHTER,* who says, he has divided a

* RICHTER *Med. Doctor. et Professor publicus Ordinarius Soc. Reg. Scient. Gotting. et Acad. Reg. Scient. Suecia Mem. in Novis Comment. Soc. Reg. Gotting.* T. vi. *ad annum* 1775.

thickened cornea, and the vessels in its substance have poured out red blood.

The cornea is not only capable of uniting by the first intention, inflaming, and suppurating, but when the inflammation is carried to a great height, a portion of its substance is sometimes removed by ulceration, and the ulcer so formed is filled up by coagulating lymph, which afterwards becomes cornea, acquiring the necessary property of transparency. This new formed part is weaker than the rest of the cornea, and commonly projects beyond it, forming one species of staphyloma; in the substance of the cornea, round the basis of the staphyloma, I have frequently seen vessels carrying red blood.

From the opinion of the cornea being devoid of life, the opacities which are found to take place on it have been considered apart from common surgery, and entrusted to the care of men who are supposed to have made the diseases of the eye their particular study.

According to this theory, the opacity was supposed to arise from a film of inanimate matter laid over the cornea, and upon that idea very acrid and irritating applications were employed with the view of scraping it off, or destroying it, as powdered glass, powdered sugar, &c. and such applications being of service, confirmed the opinion which gave rise to the practice.

Having shown that the cornea is possessed of life, I shall now point out the parts of the body it resembles in structure, and to which it bears the greatest analogy, both in its healthy actions, and those arising from disease; and endeavour, by comparing them, to establish some general principle which will explain the beneficial effects of irritating applications in cases of inflammation and opacity of the cornea.

The cornea, from some experiments and observations mentioned in a former lecture, appears to be similar in structure and use to the elastic ligaments. It has all the common properties of ligaments, those of elasticity and transparency being superadded.

Like other ligaments it can be divided into laminæ, in an healthy state has no vessels carrying red blood, and is devoid of sensibility; when divided it readily admits of union, when inflamed acquires a great degree of sensibility, is slow in its powers of resolution, and when the inflammation subsides, the coagulating lymph deposited in the adhesive stage of the inflammation remains, producing an opacity which it is afterwards found difficult to remove.

All ligamentous parts, of which I consider the cornea to be one, are weak in their vital powers; this arises from their having no vessels carrying red blood; when they inflame, which is a state of increased action, they therefore require a different mode of treatment from the other parts of the body, whose vital powers are strong, in consequence of being largely supplied with red blood.

The truly healthy inflammation requires an increased action in the parts affected; and if this, either from weakness or indolence, is not kept up, the inflammation does not go rapidly through its stages, but remains in a state between resolution and suppuration. In ligamentous structures the actions must therefore be roused and supported when under inflammation, to promote resolution, and prevent the parts from falling into an indolent diseased state. This is, however, attended with difficulty, and they too often become considerably thickened

by a deposition of coagulating lymph during the adhesive state of inflammation, which in the cornea renders it opaque. The thickening of the parts remains after the inflammation is gone, and can only be removed by absorption, which is best effected by the application of very stimulating medicines.

Upon these principles all ligamentous structures require a treatment peculiar to themselves, which may be illustrated both in inflammations of joints and of the cornea of the eye; the applications made use of with the greatest advantage in both cases being of a very stimulating kind.

The advantages attending this mode of treating the cornea were, probably, discovered by accident; and when they were ascertained, it established itself as a very general practice. It must, however, in the hands of those who had no general principle to direct their practice, have been sometimes applied without benefit, and must sometimes have been injurious.

It is an extremely curious circumstance, and probably the most so that can be met with in the history of medicine, that a local application should have been discovered to be of service in a particular disease 2513 years ago, that the same application, or those of a similar kind, should have been in very general use ever since, and in all that time no rational principle on which such medicines produced their beneficial effects should have been ascertained. This appears, from the following account, to have been the case with respect to stimulating applications to the cornea in a diseased state, and can only be accounted for by a want of knowledge of the structure of the parts, which is an argument of uncommon weight in favour of the study of anatomy.

In the Apocrypha we find, in the book of Tobit*, a very circumstantial account of an opacity of the cornea successfully treated by stimulating applications. It is there stated as a miracle, but we have the authority of JEROME, a father of the church, who wrote in the fourth century, to say, "the church "reads the books of Tobit, &c. for examples of life and in-"struction of manners, but doth not establish any doctrine by "them." We shall therefore consider the account which is given in extracts from the book of Tobit in that view.

Tob. chap. vi. ver. 2.

"When TOBIAS went down to wash himself in the river "Tigris, a fish leaped out of the river and would have devoured "him.

"Ver. 4. The angel of the Lord told him to take out the "gall, and put it up in safety.

"Ver. 6. TOBIAS asked the angel what was the use of the "gall.

"Ver. 8. As for the gall (said the angel) it is good to anoint "a man who hath whiteness in his eyes, and he shall be "healed."

Chap. xi. ver. 11.

"TOBIAS took hold of his father, and strake of the gall in "his father's eyes, saying, be of good hope, my father.

"Ver. 12. And when his eyes began to smart he rubbed "them.

"Ver. 13. And the whiteness pilled away from the corners

* TOBIT was of the tribe of Naphtali, in the city of Thisbe, in Upper Galilee; he was carried captive to Nineveh, after the extinction of the kingdom of Israel, by Enemassar, or Salmanessar, about the year of the world 3283.

GRAY's Key to the Old Testament and Apocrypha, page 354.

"of his eyes, and when he saw his son he fell upon his "neck."*

In conversing with my friend Dr. RUSSELL on the manner in which the Arabians treat inflammations and opacities of the cornea, he very kindly favoured me with the following account.

"Respecting the practice of the Arabians in disorders of the "eyes, I find nothing of consequence in my papers. An ocu- "list among them is a distinct profession; and the collyria they "apply are secret compositions, which pass hereditarily from "father to son. The Arabian writers give a number of recipes, "most of which are taken from GALEN and the Greek physi- "cians. One composition in AVICENNA contains the gall of a "crow, crane, partridge, goat, &c. At Aleppo, the gall of "the sheet fish, Silurus Glanis of LINN. was in particular re- "quest; but it should be remarked, that they always add to the "gall other ingredients, it being a material circumstance in "that country, that a recipe should consist of a multitude of "ingredients. What often struck me in their practice was the "successful application of sharp or acrid remedies, at a time "I should have been induced to make use of the mildest emol- "lient applications."

* Since this paper was read before the Royal Society, my friend Dr. WELLS acquainted me with the following case, published in the Annual Register for the year 1768.

"One of the Paris newspapers gives an account of an extraordinary cure effected by "the gall of a barbel, in a case of blindness, in substance as follows: A journeyman "watchmaker, named CENSIER, having heard that the gall of a barbel was the remedy "which TOBIAS employed to cure his father's blindness, resolved to try its effects on "the widow GERMAIN, his mother-in-law, whose eyes had for six months been af- "flicted with ulcers, and covered with a film, which rendered them totally blind:

From this account given by Dr. RUSSELL there can be no doubt of gall having continued in use, as an application to the eye among the eastern nations, from the time of TOBIT down to the present day.

I have in the course of the last three years made many trials of the effects of gall, as an application to the cornea in a diseased state. I have used it pure, and diluted; and compared its effects with those of the unguentum hydrargyri nitrati, and the solution of the argentum nitratum; and find in old cases of opacity it is, in some instances, the best application. The gall of quadrupeds, in these trials, gave more pain than the gall of fish. The painful sensation was very severe for an hour or two, and then went off. It is proper to observe, that the beneficial effects it produces appear to be in proportion to the local violence at the time of its application.

To enter further into the practical part of the treatment for removing opacities from the cornea, would be foreign to the pursuits of this learned Society, which I consider to be confined

" CENSIER having obtained the gall of that fish, squeezed the liquor out of it into a " phial, and in the evening he rubbed it with the end of a feather into his mother's eyes. " It gave her great pain for about half an hour, which abated by degrees, and her eyes " watered very much: next morning she could not open them, the water as it were " gluing her eyes up: he bathed them with pure water, and she began to see with " the eye which had received the most liquor. He used the gall again in the evening; " the inflammation dispersed, the white of her eyes became red, their colour re- " turned by degrees, and her sight became strong. He repeated it a third time, with " all the desired success. In short, she recovered her sight without any other remedy. " The widow GERMAIN is in her fifty-third year. She had been pronounced blind " by the surgeons of the Hôtel-Dieu: and her blindness and cure have been attested " by order of the lieutenant general of police. She sees stronger and clearer now than " before the accident." Annual Register, Vol. xi. page 143.

to the general principles of the different branches of science, and to collecting facts out of which new principles may be formed, or those already known better established.

The practice of applying very stimulating applications to the cornea has stood the test of twenty-five centuries, it can therefore require no support. The object of the present observations has been to explain the principle upon which the beneficial effects depend, a knowledge of which may serve as a guide to regulate our practice. It will guard us against using such medicines while the inflammatory action is increasing, it will lead us to adopt them the moment the inflammation appears to be at a stand, and not postpone this practice till an indolent unhealthy state takes place, which too often terminates in opacities no applications can afterwards remove.

II. *Observations on horizontal Refractions which affect the Appearance of terrestrial Objects, and the Dip, or Depression of the Horizon of the Sea.* By Joseph Huddart, *Esq. F. R. S.*

Read November 24, 1796.

THE variation and uncertainty of the dip, in different states of the air, taken at the same altitude above the level of the sea, was the occasion of my turning my thoughts to this subject; as it renders the latitude observed incorrect, by giving an erroneous zenith distance of a celestial object.

I have often observed that low lands and the extremity of head lands or points, forming an acute angle with the horizon of the sea, and viewed from a distance beyond it, appear elevated above it, with an open space between the land and the sea. The most remarkable instance of this appearance of the land I observed at Macao, for several days previous to a typhoon, in which the Locko lost her topmasts in Macao roads; the points of the islands and low lands appearing the highest, and the spaces between them and the sea the largest, I ever saw. I believe it arises, and is proportional to the evaporation going on from the sea; and in reflecting upon this phænomenon, I am convinced that those appearances must arise from refraction, and that instead of the density of the atmosphere increasing to the surface of the sea, it must decrease from some space above it; and that evaporation is the

principal cause which prevents the uniformity of density and refraction being continued, by the general law, down to the surface of the earth: and I am inclined to believe, though I mention it here as a conjecture, that the difference of specific gravity in the particles of the atmosphere may be a principal agent in evaporation; for the corpuscles of air, from their affinity with water, being combined at the surface of the fluid from expansion, form air specifically lighter than the drier atmosphere; and therefore float, or rise, from that principle, as steam from water; and in their rising (the surrounding corpuscles from the same cause imbibing a part of the moisture), become continually drier as they ascend, yet continue ascending until they become equally dense with the air.* However, these conjectures I shall leave, and proceed to the following observations upon refractions.

In the year 1793, when at Allonby, in Cumberland, I made some remarks on the appearance of the Abbey Head, in Galloway, which in distance from Allonby is about seven leagues; and from my window, at fifty feet above the level of the sea at that time of tide, I observed the appearance of the land about the Head as represented in Tab. I. fig. 1. There was a dry sand, *x y*, called Robin Rigg, between me and the Head, at the distance from my house of between three and four miles, over which I saw the horizon of the sea, H O; the sand at this time was about three or four feet above the level of the sea.

* Mr. HAMILTON, in his very curious Essay on the Ascent of Vapours, does not allow of this principle, even as an assistant; though by a remark (page 15) he takes notice of those appearances in the horizon of the sea, and says they arise from a strong or unusual degree of refraction; the contrary of which I hope to illustrate in the course of this paper.

The hummock *d* is a part of the head land, but appeared insulated or detached from the rest, and considerably elevated above the sea, with an open space between. I then came down about twenty-five feet, when I had the dry sand of Robin Rigg, *x y*, in the apparent horizon, and lost all that floating appearance seen from above, and the Abbey Head appeared every where distinct to the surface of the sand; this being in the afternoon, the wet or moisture on the sand would in a great measure be dried up. I have reason, therefore, to conclude that evaporation is the cause of a less refraction near the surface of the sea; and when so much so as to make an object appear elevated wholly above the horizon, (as at *d* in fig. 1.) there will from every point of this object issue two pencils of rays of light, which enter the eye of the observer; and that below the dotted line A B (parallel to the horizon of the sea H O), the objects on the land will appear inverted.

To explain this phænomenon, I shall propose the following theory, and compare it with the observations which I have made. Suppose H O, fig. 2. to represent the horizontal surface of the sea, and the parallel lines above it, the lamina or strata of corpuscles, which next the fluid are most expanded, or the rarest; and every lamina upwards increasing in density till it arrive at a maximum (and which I shall in future call the maximum of density) at the line D C, above which it again decreases in density *ad infinitum*.

Though this in reality may be the case, I do not wish to extend the meaning of the word density farther, than to be taken for the refractive power of the atmosphere; that is, a ray of light entering obliquely a denser lamina to be refracted towards a perpendicular to its surface; and in entering a rarer lamina,

the contrary; which laminæ being taken at infinitely small distances, the ray of light will form a curve, agreeable to the laws of dioptrics.

In order to establish this principle in horizontal refractions, I traced over various parts of this shore at different times, when those appearances seemed favourable, with a good telescope, and found objects sufficient to confirm it; though it be difficult at that distance of the land to get terrestrial objects well defined so near the horizon, as will afterwards appear.

One day observing the land elevated, and seeing a small vessel at about eight miles distance, I from my window directed my telescope to her, and thought her a fitter object than any other I had seen for the purpose of explaining the phænomena of these refractions. The telescope was forty feet above the level of the sea. The boat's mast about thirty-five feet, she being about twenty to thirty tons burthen. The barometer at 29,7 inches, and FAHRENHEIT's thermometer at 54°.

The appearance of the vessel, as magnified in the telescope, was as represented in fig. 8, and from the mast head to the boom was well defined. I pretty distinctly saw the head and shoulders of the man at the helm; but the hull of the vessel was contracted, confused, and ill defined: the inverted image began to be well defined at the boom (for I could not clearly perceive the man at the helm inverted), and from the boom to the horizon of the sea the sails were well defined, and I could see a small opening above the horizon of the sea, in the angle made by the gaff and mast; and had the mast been shorter by ten feet (to the height of *y*), the whole would have been elevated above the horizon of the sea, and from *y* to *d* an open space. This drawing was taken from a sketch I took at the

time, and represents the proportion of the inverted to the erect object, as near as I could take it by the eye, the former being about two-thirds of the latter in height, and the same breadth respectively; though at one time during my observation, which I continued for about an hour, I thought the inverted nearly as tall as the erect object. The day was fine and clear, with a very light air of wind, and I found very little tremor or oscillation in viewing her through the telescope.

I have laid down fig. 4. for the explanation of the above phænomena, in which A represents the window I viewed B the vessel from; H O, the curved surface of the sea; C D parallel to H O, the height of the maximum of density of the atmosphere; the lines marked with the small letters *a a*, *b b*, *c c*, *d d*, the pencils of rays under their various refractions from the vessel to the eye, or object glass of the telescope.

The pencil of rays *a a*, from a point near the head of the mainsail, is wholly refracted in a curve convex upwards, being every where above the maximum of density; and the pencil of rays *d d*, which issues from the same point in the sail, and passes near the horizon of the sea at *x*, is convex upwards from the sail to W, where it passes the line of maximum of density, which is the point of inflection; there it becomes convex downwards, passing near the horizon at *x* to *y*, where it is again inflected, and becomes convex upwards from thence to the eye. The pencil of rays *b b*, from the end of the boom, passing nearly parallel to the horizon, and near the maximum of density, suffers very little deviation from a right line in the first part; but in ascending (from the curvature of the sea) will be convex upwards to the eye. The pencil of rays *c c*, from the same point in the boom, may have the small part to *c* convex upwards,

from *c* to *z* it will be convex downwards, and from *z* to the eye convex upwards.

From this investigation it appears, that two pencils of rays cannot pass from the same point, and enter the eye, from the law of refraction, except one pencil pass through a medium which the other has not entered; and therefore the maximum of density was below the boom, and could not exceed ten feet of height above the surface of the sea at the time these observations were made.

Respecting the hull of the vessel being confused, and ill defined in the telescope, as by fig. 3, it arises from the blending of the rays, from the different parts of the object, refracted through the two mediums; some parts of the hull appearing erect, and some inverted. Suppose the dotted line *ii*, fig. 4, an indefinite pencil of rays, passing from between the inverted and erect parts of the object, or the upper part of the hull of the vessel, to the eye, (for the lower part of the hull could not be observed): the objects cannot appear inverted, except the angles at the eye *a*A*c* and *a*A*d*, exceed the angle *a*A*i*; for the intermediate space could only be contracted by the secondary pencils of rays. The lengths of the inverted, compared with the erect image of the sail, is as the sines of the angles at the eye *a*A*i* to *i*A*d*; and the angle at the eye *a*A*d*, made by the two pencils of rays from the same point near the head of the sail, must be double the angle *a*A*i*, when the inverted image is as tall as the erect. In this case, the sines of the angles *a*A*b*, *a*A*c*, *a*A*d*, fig. 4, are proportional to the altitudes *a b*, *a c*, *a d*, in the magnified view of the vessel, fig. 3.

Under this consideration no inverted image of the sail will be formed, until the angle at the eye, made by the two refracted

pencils of rays *a a* and *d d*, exceed the angle made by *a a*, and *b b*, the apparent height of the sail of the vessel; for were those angles equal, the inverted sail would only be contracted into the parallel of altitude of the boom *b*, and render the appearance confused, as in the hull of the vessel.

Respecting the existence of two pencils of rays entering the eye from every point of an object not more elevated than *a*, or less than *i*, fig. 3, in this state of the atmosphere, I cannot bring a stronger proof than that of the strength of a light when the rays pass near the horizon of the sea, proved by the following observations.

Going down Channel about five years ago in the Trinity yacht, with several of the elder brethren, to inspect the light-houses, &c. I was told by some of the gentlemen, who had been on a former survey, that the lower light of Portland was not so strong as the upper light, at near distances, but that at greater distances it was much stronger. I suspected that this difference arose from the lower light being at or near the horizon of the sea, and mentioned it at the time; but afterwards had a good opportunity of making the observation. We passed the Bill of Portland in the evening, steering towards the Start, a fresh breeze from the northward and clear night; when we had run about five leagues from the lights, during which time the upper light was universally allowed to be the stronger, several gentlemen keeping watch to make observations thereon, the lower light, drawing near the horizon, suddenly shone with double lustre. Mr. STRACHAN, whose sight is weak, had for some time before lost sight of both lights, but could then clearly perceive the lower light. I then went aloft, (as well as others,) but before I got half mast up, the lower

light was weaker than the upper one; on coming down upon deck, I found it again as strong as before. We proceeded on, and soon lost the lower light from the deck; and upon drawing the upper light near the horizon, it like the former shone exceeding bright. I again went aloft, when it diminished in brightness; but from the mast head I could then see the lower light near the horizon as strong as before. This is in consequence of the double quantity of light entering the eye by the two pencils of rays from every point. To illustrate which, we compare the vessel, fig. 4, to a lighthouse built upon the shore, and A the place of the observer; and having brought down the light so low as to view it in the direction *a a*, another light would appear in the horizon at *x* from the pencil *d d*; and had the vessel been still enough to have observed it at this time with a good glass, I doubt not but the two images might have been distinctly seen: as the light dropped, (by increasing the distance) the two images would appear continually to approach each other, till blended with double light in one, and disappear at the altitude *i*, above the apparent horizon of the sea. But, as explained before, if the strength of evaporation did not separate by refraction the pencils *a a* and *d d* to a greater angle than double the angle that the lamps and reflectors appear under, the two images would be blended, and the strong appearance of light would be of shorter duration. The distance run from the lights, during the time each of the lights shone bright, would have been useful, but this did not occur at the time, nor have I had the like opportunity since. However, I recommend to the mariner to station people at different heights in looking out for a light, in order to get sight of it near the horizon, when it is always strongest.

Respecting the appearance of the Abbey Head before mentioned, fig. 1, the dotted line A B represents the limit, or the lowest points of the land that can be seen over the sea; for, as above stated, all the objects appearing below this line, are the land above it inverted; and where the land is low, as at *d* and *m*, it must appear elevated above the horizon of the sea.

In fig. 5. let H O represent the curve of the ocean, and *d* the extreme top of the mount visible at A by the help of refraction; the dotted pencil of rays *c c* passing from *d* to the eye in some part a little below the maximum of density, where inversion begins; therefore no land lower than this can be seen; for any pencil from a point in the land lower than this, must in the refraction have a contrary flexure in the curve, and therefore pass above the observer. Let A D be a tangent to the curve at A, then the object *d* will appear to be elevated by refraction to D; also let A *v* be a tangent to the pencil A *x* at A, then the angle D A *x* will appear to be an open space, or between D and the horizon of the sea. Suppose a star should appear very near and over the mount *d*, as at *, two pencils would issue from every point of it, and form a star below as well as above the hummock *d*. There are always confused or ill defined images of the objects at the height of the dotted line, fig. 1, above the level of the sea, as before mentioned; and instead of the points of *d* ending sharp in that line, they appear blunted, and the Abbey Head is frequently insulated at the neck *m*.

I have viewed, from an elevated situation, a point or head land at a distance beyond the horizon of the sea, forming, as in fig. 6. a straight line A B, making an acute angle B A O with the horizon of the sea. Seeing the extreme point blunted and elevated, I descended; and though in descending the horizon

cut the land higher, as at H O, H O, yet the point had always the same appearance as *a*, *a*, *a*, fig. 6, though the land is known to continue in the direction of the straight line A B to beneath the horizon, or nearly so, as viewed from the height above.

If then from a low situation we view this head land through a telescope, the inclination of the surface A B to the horizon being known to be a straight line, it will appear as in fig. 7. the dotted line (at the height of the point where a perpendicular *x y* would touch the extreme of the land) being at the limit or lowest point of erect vision. And if a tangent to the curved appearance of the land *a b*, is drawn parallel to the inclined surface of the land A B, fig. 6, touching it at C, the point C will shew the height of the maximum of density, where the pencil of the rays of light, from thence to the eye, approach nearest the sea; for pencils of rays from this land, taken at small distances from C, will form parallel curves, nearly, through the refracting mediums, and C will be the point of greatest refraction; for above C as at B the refraction somewhat decreasing, will appear below the line *a b*, or the parallel to the surface of the land, and the refractions decrease below the point C; for had they increased uniformly down to the surface of the sea, it would render the apparent angle of the point of land *z* more acute than the angle C *a* O, contrary to all observations.

Thus I have endeavoured to explain the phænomena of the distorted appearance of the land near the horizon of the sea, when the evaporation is great; and when at the least, I never found the land quite free from it when I used a telescope; and from thence infer, that we cannot have any expectation to find a true correction for the effect of terrestrial refraction, by tak-

ing any certain part of the contained arc; for the points z C B, fig. 7, will have various refractions, though they are at nearly the same distance from the observer. And if the observations are made wholly over land, if the ground rises to within a small distance of the rays of light in their passage from the object to the eye, as well as at the situation of the object and observer, the refractions will be subject to be influenced by the evaporation of rains, dews, &c. which is sufficiently proved by the observations of Colonel WILLIAMS, Captain MUDGE, and Mr. DALBY, Phil. Trans. 1795, p. 583.

The appearances mentioned by Colonel WILLIAMS, Captain MUDGE, and Mr. DALBY, (Phil. Trans. 1795, p. 586, 587,) cannot be demonstrated upon general principles, as they arise from evaporation producing partial refractions. In those general principles, it is supposed that the same lamina of density is every where at an equal distance from the surface of the sea, at least as far as the eye can reach a terrestrial object; but in the partial refractions, the lamina of the expanded or rarefied medium may be of various figures according to circumstances, which will refract according to the incidence of the rays, and affect the appearance of the land accordingly, which I have often seen to a surprising degree. But my principal view is to shew the uncertainty of the dip of the sea, and that the effect of evaporation tends to depress the apparent horizon at *x*, when the eye is not above the maximum of density; and from hence the difficulty of laying down any correct formula for these refractions, whilst the law of evaporation is so little understood, which indeed seems a task not easy to surmount. The effect indicated by the barometer and thermometer is insufficient: and should the hygrometer be improved to fix a

standard for moisture in the atmosphere, and shew the variations near the surface of the ocean, which certainly must be taken into the account, (evaporation going on quicker in a dry than a moist atmosphere,) the theory might still be incomplete for correcting the tables of the dip. I shall therefore conclude this paper, by shewing a method I used in practice, in order to obviate this error, in low latitudes.

When I was desirous to attain more accurately the latitude of any head land, &c. in sight, I frequently observed the angular distances of the sun's nearest limb from the horizons, upon the meridian both north and south, beginning a few minutes before noon, and taking alternately the observations each way, from the poop, or some convenient part of the ship, where the sun and the horizon both north and south were not intercepted; and having found the greatest and least distances from the respective horizons, which was at the sun's passing the meridian, and corrected both for refraction, by subtracting from the least, and adding to the greatest altitude, the quantity given by the table; and also having corrected for the error of the instrument, and the sun's semidiameter; the sum of these two angular distances, reduced as above, — 180°, is equal to double the dip, as by the following

EXAMPLE.

The sun's declination 4° 32′ 30″ north, and its semidiameter 15′ 58″ took the following observation:

The meridian distance of the sun's nearest limb from the horizon of the sea -	South. 78° 36′ 30″	=	North. 101° 1′ 20″
Refraction *per* table - -	— 0 11	=	+ 0 11
Distances corr. for refraction =	78 36 19	=	101 1 31
Error of the sextant - -	+ 1 32		+ 1 32
Sun's semidiameter - -	+ 15 58		+ 15 58
	78 53 49		101 19 1
½ diff. or the dip found - -	— 6 25		78 53 49
Altitude reduced - =	78 47 24		180 12 50
Zenith distance - - =	11 12 36		180
			Diff. 12 50
The sun's declination N. =	4 32 30		½ = 6 25
			Dip.
Latitude of the ship N. =	15 45 06		

I regret that I cannot in this paper insert the dip which I have found in my observations; for I only retained the latitude of the ship determined thereby, as is usual at sea; I generally rejected the error of the instrument, the dip, and semidiameter, as they affect both observations with the same signs, and reduced the observation by the following method:

	South:	North.
Sun's dist. as before	78° 36′ 30″	101° 1′ 20″
Refraction -	— 0 11	+ 0 11
Dis. corr. for refraction	78 36 19	101 1 31
		101° 1′ 31″
	+ 78 36 19	
Sum of S. diam. dip, and refraction = ½ diff.	Sum 179 37 50	
	+ 11 5	180
		+ 11 5
	78 47 24	Diff. 22 10
		½ 11 5
		101 12 36
	90	90
The ½ dist. as before =	11 12 36	½ D. = 11 12 36

It may be observed, that neither the dip, semidiameter, or index error, can affect the zenith distance of the sun's centre; and the refraction being small near the zenith, the result must be true if the angles are accurately taken; and it is only necessary to observe, that when the sum of the distances is less than 180°, the half difference must be added to the distances, as by the last reduction. There is a difficulty in making this observation when the sun passes the meridian very near the zenith, as the change in azimuth from east to west is too quick to allow sufficient time; nor can it be obtained by the sextant when the sun passes the meridian more than 30 degrees from the zenith; for I never could adjust the back observation of the HADLEY's quadrant with sufficient accuracy to be depended upon.

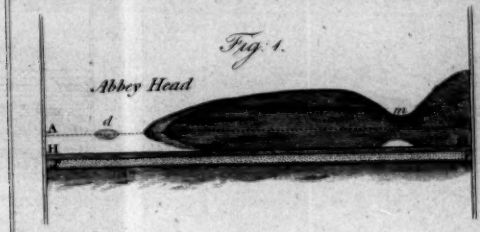
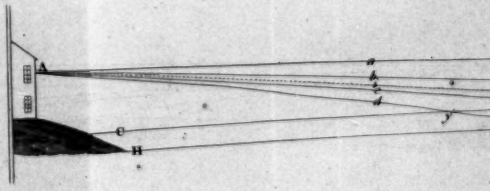


Fig. 1.
Abbey Head
A
H
d
m


A
a
b
c
d
e
H
y

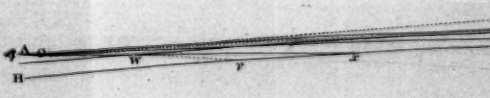

A
H
w
p
x

Fig. 6.
H
H
H
A

Fig. 2.

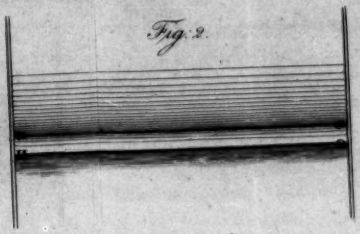


Fig. 3.

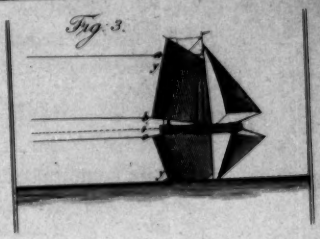


Fig. 4.

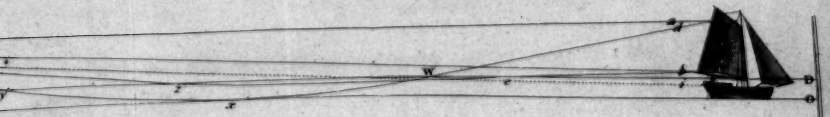
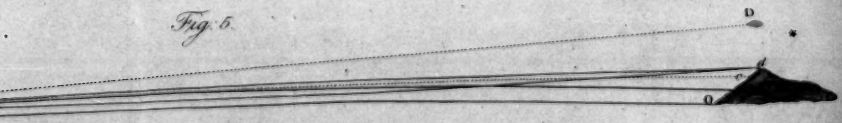


Fig. 5.



6.

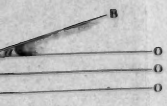


Fig. 7.

III. *Recherches sur les principaux Problèmes de l'Astronomie Nautique. Par Don* Josef de Mendoza y Rios, *F. R. S. Communicated by Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read December 22, 1796.

DANS les Recherches suivantes, je me suis proposé de considérer les principaux problèmes de l'Astronomie Nautique d'une manière générale, pour établir des formules qui embrassent tous les cas, et dont on puisse déduire les différentes méthodes propres à les résoudre avec plus ou moins d'avantages. Elles sont divisées en deux Parties.

Dans la Première Partie j'ai compris ce qui regarde la détermination de la latitude du lieu du vaisseau par deux hauteurs du soleil; ainsi que le calcul de l'angle horaire d'un astre par la hauteur observée, et celui de la hauteur par l'angle horaire.

Le sujet de la Seconde Partie est la réduction des distances de la lune au soleil, ou à une étoile, observées à la mer, pour déterminer la longitude. J'ai considéré séparément les solutions directes, et les méthodes d'approximation. Quant aux dernières, j'ai tâché aussi de donner des formules propres pour examiner et porter un jugement définitif sur tous les procédés de cette espèce dont on voudra prouver la fausseté ou la justesse, ou bien les dégrés d'exactitude qu'ils comportent.

Dans ces Recherches, ainsi que dans un ouvrage * que j'ai composé, avec un grand nombre de tables pour faciliter les calculs de l'Astronomie Nautique, j'ai employé les sinus-verses en

* L'impression de cet ouvrage est déjà très avancée

les envisageant sous certaines rélations réciproques qui me paroissent susceptibles de plusieurs applications utiles. Avant d'entrer en matière, il est donc à-propos de les expliquer, et de faire connoître les expressions dont je me suis servi pour les désigner. Les voici, (en supposant, comme nous le ferons par la suite, le sinus total = 1)

sinus-verse $A = 1 - \cos. A = 2 \sin.^2 \frac{1}{2} A$

susinus-verse $A = 1 + \cos. A = \text{sin. v.} (180° - A) = 2 \cos.^2 \frac{1}{2} A$

cosinus-verse $A = 1 - \sin. A = \text{sin. v.} (90° \sim A) =$
$\text{susin. v.} (90° + A) = 2 \sin.^2 \frac{1}{2} (90° \sim A) =$
$2 \cos.^2 \frac{1}{2} (90° + A)$

sucosinus-verse $A = 1 + \sin. A = \text{sin. v.} (90° + A) =$
$\text{susin. v.} (90° \sim A) = 2 \sin.^2 \frac{1}{2} (90° + A) =$
$2 \cos.^2 \frac{1}{2} (90° \sim A)$

PREMIÈRE PARTIE.

Trouver la Latitude du Vaisseau par deux Hauteurs du Soleil, et le Tems écoulé entre les Observations.

La latitude est l'élément le plus précieux de la Navigation. La facilité et l'exactitude avec lesquelles on peut la déduire de la hauteur méridienne du soleil, sont cause que les Pilotes se fient principalement à cette donnée pour la direction de leurs routes. Mais celà même fait, que, quand on manque l'observation du midi, l'incertitude qui y résulte est plus grande; et le danger devient imminent dans des circonstances critiques. Ainsi, depuis que les voyages longs et fréquens de la Navigation moderne donnèrent lieu à des recherches exactes pour traverser l'Océan avec sureté, on a tâché de trouver des

régles propres pour déterminer la latitude par des observations prises hors du méridien; et le public possède à ce sujet un grand nombre de méthodes,* plus ou moins ingénieuses dans la théorie, mais dont la plupart sont restées tout à fait inutiles dans

* Le célèbre PIERRE NUNNEZ (ou NONIUS) s'occupa beaucoup des moyens de déterminer la latitude, et après avoir démontré la fausseté des régles publiées par PIERRE APPIAN (*Cosmographia*) et JACOB ZIEGLER (*Commentarium in secundum librum Naturalis Historiæ Plinii*) il donna différens problèmes de son invention, et entre eux celui qu'on résout par deux hauteurs, et l'arc d'horizon compris par les verticaux de l'astre (*De Arte atque Ratione Navigandi*, 1573; *De Observ. Regul. et Instrum. Geometr. &c.*). Je n'ai pas pu éclaircir celui qui le premier substitua au lieu du dernier élément, l'arc de l'équateur compris entre les horaires, ou bien l'intervalle de tems entre les observations; mais on trouve cette solution énoncée comme une chose connue quoique peu utile, dans le traité *De Globis et eorum Usu*, par ROBERT HUES. (Je n'ai jamais vu la première édition de ce livre; celles que je connois, outre les traductions en Anglois et en François, sont une *cum Annott.* J. ISAACCI PONTANI; *Amst.* 1617; et une autre, *Oxon*, 1663.) Le procédé mentionné par HUES exige l'usage des globes. M. FACIO DUILLIER (*Navigation improved*, 1728,) expliqua avec assez de détail la même méthode par le calcul trigonométrique; et cependant M. PITOT la publia ensuite (*Mém. de l'Académie des Sciences de Paris*, 1736,) comme quelque chose d'important et de nouveau. Mr. R. GRAHAM imagina pour le même objet un appareil méchanique (*Philosoph. Transact.* 1734,); et M. DE MAUPERTUIS donna aussi une solution tirée des formules établies dans son *Astronomie Nautique* (*Probl. XII.*). Dans les ouvrages postérieurs on ne rencontre, pour la plupart, que les idées des auteurs que nous venons de citer. Au reste, voyez sur la détermination de la latitude par deux hauteurs et par d'autres procédés; *Comm. Acad. Imp. Sc. Petropolit.* 1729, Mémoires de DAN. BERNOUILLI, HERMAN, EULER, FR. CHRIST. MAYER, et W. KRAFFT. Id. 1779, Mémoire de M. LEXELL; *Nautical Almanack*, 1778, Appendice par M. LYONS; *L'Astronomie des Marins, par le* P. PÉZENAS; *L'Astronomie de* M. DE LA LANDE; RÖSLERS *Handbuch der Practic. Astronomie*; *La Trigonométrie rectiligne et sphérique, par* M. CAGNOLI; *Berlin. Astronom. Jahrbücher*, 1787, 1789, 1790, Mémoires de M. M. HENERT, GRAF PLAATEN, et SCHUBERT; *Allgemeine Wörterbuch der Marine*, par RÖDING; *Sammlung Astronom. Abhandlungen*, par KÄSTNER; *Elements of Navigation, by* ROBERTSON; *Traité de Navigation de* BOUGUER, *par* LA CAILLE; *Opuscules Mathématiques de* M. D'ALEMBERT, IV. p. 357; *Cours de Mathématiques*,

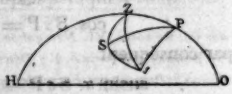
la pratique. La seule qui ait été adoptée par les navigateurs assez généralement est celle de M. DOUWES,* qui mérita pour sa solution une récompense du Bureau des Longitudes de la Grande Bretagne. Cette méthode, pourtant, est sujette à quelques inconvéniens; entre autres celui d'exiger dans les opérations l'usage combiné des nombres naturels et artificiels. Je me suis proposé de trouver des moyens plus simples et plus généraux pour calculer la latitude; ce qui m'a engagé dans des recherches, dont je me contenterai de donner ici celles qui me paroissent remplir quelque but utile.

par M. BÉZOUT, VI. *Navigation*; *Voyage de la Flore, par* M. M. DE VERDUN, DE BORDA, et PINGRÉ, I.; *Description et Usage du Cercle de Réflexion, par* M. DE BORDA; *Dictionnaire Encyclopédique des Mathématiques*, II.; *Traité Analytique des Mouvemens apparens des Corps Célestes, par* M. DU SÉJOUR, &c.

* M. CORNELIUS DOUWES expliqua sa méthode avec beaucoup de détails théoriques et pratiques dans les *Actes de l'Académie de Haarlem*, I. 1754. Ce Mémoire est très intéressant, mais il est resté presque tout à fait inconnu au reste de l'Europe, à cause de la langue du pays où il fut écrit. Je me propose de publier la traduction en François. Les tables de M. DOUWES pour faciliter sa méthode suivirent de très près le précédent ouvrage; et c'est d'après un exemplaire de cette édition que HARRISON fit la sienne en 1759, à Londres. Le Dr. PEMBERTON, à la vue de ces tables, dont il paroit avoir ignoré l'auteur, trouva la théorie et l'inséra dans les *Transactions Philosophiques*, 1760. La connoissance qu'on a des principes du professeur Hollandois est pour la plupart dérivée de ce Mémoire. Sur cette méthode, et sur quelques changements qu'on y a proposé ou fait, ainsi que sur les tables plus étendues qu'on a calculé pour en faciliter l'usage, voyez d'ailleurs—*The British Mariner's Guide, by* Mr. MASKELYNE; *Nautical Almanack*, 1771, Appendice par l'Amiral CAMPBELL; *Nautical Almanack*, 1781, Appendice par Mr. EDWARDS; *Requisite Tables*, 1781; *Le Guide du Navigateur, par* M. LEVÊQUE; *Sammlung Astronomischer Abhandlungen*, 1793, par M. BODE, Mémoire de Mr. NIEUWELAND; *Verhandeling over het bepaalen der Lengte op Zee, Amst.* 1789, par M. M. VAN SWINDEN, NIEUWELAND, et VAN KEULEN; *L'Astronomie de* M. DE LA LANDE; *Tratado de Navegacion, por* Don JOSEF DE MENDOZA RIOS, 1787; *Connoissance des Tems*, 1793, Mémoire de M. DE MENDOZA; *Nautical Almanacks*, 1797—1800, Appendice par Mr. BRINKLEY, &c.

Nous supposerons, pour la Première Partie de ces Recherches, la plus grande hauteur du soleil $= a$, l'angle horaire correspondant $= b$, l'azimuth correspondant $= c$, la déclinaison correspondante $= d$, ou la distance au pole élevé $=$ D, et l la latitude du lieu où l'on a observé cette hauteur; la petite hauteur du soleil $= a'$, et les autres éléments rélatifs à cette observation $= b', c', d', D', l'$. Nous représenterons aussi l'angle horaire, moyen entre b et b', par m, et la différence entre b et b' par t.

Méthode directe.



Soit H O l'horizon, H Z P O le méridien, Z le zénith, P le pole élevé, et S, s les lieux du soleil aux instants des observations, que nous supposerons faites dans le même lieu.

Voici le procédé qu'on prescrit ordinairement pour faire le calcul par la Trigonométrie Sphérique.

Dans le triangle S P s on connoit l'angle S P s qu'on déduit de l'intervalle, et les deux cotés S P, s P qui sont les distances du soleil au pole élevé; dont on pourra conclure S s, et S s P, ou s S P. Avec S s et les compléments des hauteurs Z S, Z s on calculera Z s S ou Z S s. La comparaison entre S s P et Z s S, ou entre s S P et Z S s donnera Z s P ou Z S P. Le premier de ces deux angles, et les cotés Z s, P s suffisent pour résoudre le triangle Z s P; ou bien, on pourra résoudre le triangle Z S P à l'aide de l'autre angle Z S P et de Z S, P S; en concluant ainsi le complément de la latitude Z P.

Tâchons d'établir des formules pour abrégér et simplifier ce calcul.

Dans le triangle S P s que je considérérai comme isoscèle, en supposant la déclinaison constante,

on a $\cos. S s = \cos. t \sin.^2 D + \cos.^2 D.$

d'où l'on déduit

$$1 - \text{sin. v. } S s = \cos. t \sin.^2 D + \cos.^2 D.$$

$$\text{sin. v. } S s = \sin.^2 D - \cos. t \sin.^2 D.$$

$$\text{sin. v. } S s = \sin.^2 D \text{ sin. v. } t.$$

Formule propre pour le calcul de $S s$ par les sinus-verses.

En substituant $2 \sin.^2 \frac{1}{2} S s = \text{sin. v. } S s$, et $2 \sin.^2 \frac{1}{2} t =$ sin. v. t, on déduit, pour le calcul par les sinus

$$\sin. \tfrac{1}{2} S s = \sin. D \sin. \tfrac{1}{2} t.$$

Dans le même triangle on a

$$\cos. S s P = \frac{\cos. D - \cos. S s \cos. D}{\sin. S s \sin. D}$$

par conséquent

$$\text{susin. v. } S s P - 1 = \frac{\cos. D - \cos. S s \cos. D}{\sin. S s \sin. D}$$

$$\text{susin. v. } S s P = \frac{\cos. D - \cos. S s \cos. D + \sin. S s \sin. D}{\sin. S s \sin. D}$$

$$\text{susin. v. } S s P = \frac{\cos. D - \cos. (S s + D)}{\sin. S s \sin. D}$$

$$\text{susin. v. } S s P = \frac{2 \sin. (\frac{1}{2} S s + D) \sin. \frac{1}{2} S s}{\sin. S s \sin. D}$$

et en substituant $\sin \frac{1}{2} S s = \sin. D \sin. \frac{1}{2} t$

il résultera

$$\text{susin. v. } S s P = \frac{2 \sin. (\frac{1}{2} S s + D) \sin. \frac{1}{2} t}{\sin. S s}$$

Formule pour calculer $S s P$ par les sinus-verses, et les double-sinus.

Pour le calcul par les sinus on déduit

$$\cos. \tfrac{1}{2} S s P = \sqrt{\frac{\sin. (\frac{1}{2} S s + D) \sin. \frac{1}{2} t}{\sin. S s}}$$

On pourroit trouver aussi

$$\text{sin. v. } S s P = \frac{2 \sin. (\frac{1}{2} S s \sim D) \sin. \frac{1}{2} t}{\sin. S s}$$

$$\text{et} \quad \sin. \tfrac{1}{2} S s P = \sqrt{\frac{\sin. (\frac{1}{2} S s \sim D) \sin. \frac{1}{2} t}{\sin. S s}}$$

Dans le triangle Z S *s* on a

$$\cos.\ S\,s\,Z = \frac{\sin.\ a - \cos.\ S\,s \sin.\ a'}{\sin.\ S\,s \cos.\ a'}$$

$$1 - \text{sin. v. } S\,s\,Z = \frac{\sin.\ a - \cos.\ S\,s \sin.\ a'}{\sin.\ S\,s \cos.\ a'}$$

$$\text{sin. v. } S\,s\,Z = \frac{\sin.\ (S\,s + a') - \sin.\ a}{\sin.\ S\,s \cos.\ a'}$$

$$\text{sin. v. } S\,s\,Z = \frac{2 \cos.\ \frac{1}{2}\,(S\,s + a' + a) \sin.\ \frac{1}{2}\,(S\,s + a' - a)}{\sin.\ S\,s \cos.\ a'}$$

Formule pour calculer S *s* Z par les sinus-verses.

Pour le calcul par les sinus on déduit

$$\sin.\ \tfrac{1}{2}\,S\,s\,Z = \sqrt{\frac{\cos.\ \frac{1}{2}\,(S\,s + a' + a) \sin.\ \frac{1}{2}\,(S\,s + a' - a)}{\sin.\ S\,s \cos.\ a'}}$$

On pourroit trouver aussi

$$\text{susin. v. } S\,s\,Z = \frac{2 \sin.\ \frac{1}{2}\,(S\,s + a - a') \cos.\ \frac{1}{2}\,\big((S\,s - a') \sim a\big)}{\sin.\ S\,s \cos.\ a'}$$

et $$\cos.\ \tfrac{1}{2}\,S\,s\,Z = \sqrt{\frac{\sin.\ \frac{1}{2}\,(S\,s + a - a') \cos.\ \frac{1}{2}\,\big((S\,s - a') \sim a\big)}{\sin.\ S\,s \cos.\ a'}}$$

Plusieurs auteurs de Trigonométrie Sphérique supposent que l'angle Z *s* P est toujours égal à la différence entre S *s* P, et S *s* Z; mais cette régle générale n'est pas exacte. Le vertical Z *s* peut tomber à l'autre coté de S *s* relativement au pole élevé; ce qui a lieu quand l'astre dans sa révolution diurne passe entre le zénith et le pole élevé. On doit prendre alors la somme, et non pas la différence des angles ci-dessus, pour avoir celui qu'on cherche.

L'angle Z S P peut être aussi égal au complément à 360° de la somme de *s* S P, et Z S *s*; et l'attention à cette circonstance seroit nécessaire dans le cas où l'on feroit le calcul par les angles en S.

Après avoir déterminé l'angle Z *s* P, on a

$$\sin.\ l = \cos.\ Z\,s\,P \cos.\ a' \sin.\ D + \sin.\ a' \cos.\ D$$

$$\sin.\ l = \sin.\ a' \cos.\ D + \cos.\ a' \sin.\ D - \text{sin. v. } Z\,s\,P \cos.\ a' \cos.\ d$$

$$\text{sin. } l = \text{sin. } (D + a') - \text{sin. v. } Z\,s\,P \text{ cos. } a' \text{ cos. } d$$

$$1 + \text{sin. } l = 1 + \text{sin. } (D + a') - \text{sin. v. } Z\,s\,P \text{ cos. } a' \text{ cos. } d$$

$$\text{sucos. v. } l = \text{sucos. v. } (D + a') - \text{sin. v. } Z\,s\,P \text{ cos. } a' \text{ cos. } d$$

$$\text{sucos. v. } l = \text{sucos. v. } (D + a') \left(1 - \frac{\text{sin. v. } Z\,s\,P \text{ cos. } a' \text{ cos. } d}{\text{sucos. v. } (D + a')}\right).$$

Formule pour déterminer finalement l par les sinus-verses; car on voit, qu'en faisant $\frac{\text{sin. v. } Z\,s\,P \text{ cos. } a' \text{ cos.}^2 d}{\text{sucos. v. } (D + a')} = \text{sin. v. N}$, on aura sucos. v. $l = $ sucos. v. $(D + a')$ cos. N.

En substituant dans la formule précédente

$2 \text{ sin.}^2 \frac{1}{2} (90 + l) = \text{sucos. v. } l$, $2 \text{ sin.}^2 \frac{1}{2} Z\,s\,P = \text{sin. v. } Z\,s\,P$,

et $2 \text{ sin.}^2 \frac{1}{2} (90° + D + a') = \text{sucos. v. } (D + a')$,

il résultera, pour le calcul de l par les sinus,

$$\text{sin. } \tfrac{1}{2}(90° + l) = \text{sin. } \tfrac{1}{2}(90° + D + a') \sqrt{1 - \frac{\text{sin.}^2 \frac{1}{2} Z\,s\,P \text{ cos. } a' \text{ cos. } d}{\text{sin.}^2 \frac{1}{2} (90° + D + a')}}$$

Par où l'on voit, qu'en faisant $\frac{\text{sin. } \frac{1}{2} Z\,s\,P \sqrt{\text{cos. } a' \text{ cos. } d}}{\text{sin. } \frac{1}{2} (90° + D + a')} = \text{sin. N}$ on aura sin. $\frac{1}{2} (90° + l) = $ sin. $\frac{1}{2} (90° + D + a')$ cos. N.

On pourroit aussi déduire

$$\text{cos. v. } l = \text{cos. v. } (D + a') \left(1 + \frac{\text{sin. v. } Z\,s\,P \text{ cos. } a' \text{ cos. } d}{\text{cos. v. } (D + a')}\right)$$

pour faire $\frac{\text{sin. v. } Z\,s\,P \text{ cos. } a' \text{ cos. } d}{\text{cos. v. } (D + a')} = \text{cos. N}$, et avoir

cos. v. $l = $ cos. v. $(D + a')$ susin. v. N.

Aussi,

$$\text{cos. } \tfrac{1}{2}(90° + l) = \text{cos. } \tfrac{1}{2}(90° + D + a') \sqrt{1 + \frac{\text{sin.}^2 \frac{1}{2} Z\,s\,P \text{ cos. } a' \text{ cos. } d}{\text{cos.}^2 \frac{1}{2} (90° + D + a')}}$$

où, en faisant $\frac{\text{sin. } Z\,s\,P \sqrt{\text{cos. } a' \text{ cos. } d}}{\text{cos. } \frac{1}{2} (90° + D + a')} = \text{tan. N}$, on a

$$\text{cos. } \tfrac{1}{2} (90° + l) = \frac{\text{cos. } \frac{1}{2} (90° + D + a')}{\text{cos. N}}.$$

Nous examinerons à présent l'erreur qui résulte dans la latitude de celles qu'on peut commettre dans les élémens du calcul.

Supposons premièrement une erreur δt dans l'intervalle. Les analogies différentielles donnent, en supposant l'angle horaire et la latitude variables,

$$\delta b = \frac{\delta l\,(\text{tan. } d - \text{tan. } l \text{ cos. } b)}{\text{sin. } b}$$

et $$\delta b' = \frac{\delta l \text{ tan. } d - \text{tan. } l. \text{ cos. } b'}{\text{sin. } b'}$$

On aura donc

$$\delta t = \delta b' - \delta b = \delta l \left(\text{tan. } l\,(\text{cot. } b - \text{cot. } b') - \frac{\text{tan. } d\,(\text{sin. } b' - \text{sin. } b)}{\text{sin. } b \text{ sin. } b'} \right)$$

Par conséquent $$\delta l = \frac{\delta t \text{ sin. } b \text{ sin. } b'}{\text{tan. } l \text{ sin. } t - 2 \text{ tan. } d \text{ cos. } m \text{ sin. } \frac{1}{2} t};$$

ou bien $$\delta l = \frac{\delta t}{\text{tan. } l\,(\text{cot. } b - \text{cot. } b') - \text{tan. } d\,(\text{cosec. } b - \text{cosec. } b')}.$$

En supposant une erreur δa dans la grande hauteur, on a $-\delta t = \delta b = \frac{\delta a \text{ cos. } a}{\text{cos. } d \text{ cos. } l \text{ sin. } b}$; ce qui, étant substitué dans l'équation précédente, donne

$$\delta l = -\frac{\delta a \text{ cos. } a \text{ sin. } b'}{\text{cos. } d \text{ sin. } l \text{ sin. } t - 2 \text{ sin. } d \text{ cos. } l \text{ cos. } m \text{ sin. } \frac{1}{2} t}$$

et $$\delta l = -\frac{\delta a \text{ cos. } a \text{ sin. } b'}{\text{cos. } d \text{ sin. } l \text{ sin } t - \text{sin. } d \text{ cos. } l\,(\text{sin. } b' - \text{sin. } b)}$$

Pour l'erreur de la petite hauteur on auroit aussi

$\delta t = \delta b = \frac{\delta a' \text{ cos. } a'}{\text{cos. } d \text{ cos. } l \text{ sin. } b}$; d'où l'on déduit

$$\delta l = \frac{\delta a' \text{ cos. } a' \text{ sin. } b}{\text{cos. } d \text{ sin. } l \text{ sin. } t - 2 \text{ sin. } d \text{ cos. } l \text{ cos. } m \text{ sin. } \frac{1}{2} t}$$

et $$\delta l = \frac{\delta a' \text{ cos. } a' \text{ sin. } b}{\text{cos. } d \text{ sin. } l \text{ sin. } t - \text{sin. } d \text{ cos. } l\,(\text{sin. } b' - \text{sin. } b)}$$

Méthode indirecte, en déduisant premièrement l'Angle horaire moyen.

La Trigonométrie Sphérique donne $\text{cos. } b = \frac{\text{sin. } a - \text{sin. } d \text{ sin. } l}{\text{cos. } d \text{ cos. } l}$

et $\text{cos. } b' = \frac{\text{sin. } a' - \text{sin. } d \text{ sin. } l'}{\text{cos. } d' \text{ cos. } l'}$. Par conséquent

$$\cos. b - \cos. b' = 2 \sin. m \sin. \tfrac{1}{2} t = \left\{ \frac{\begin{array}{l} \sin. a \cos. d' \cos. l' - \sin. d \cos. d \cos. l \\ - \sin. d \cos. d' \sin. l \cos. l' \\ + \cos. d \sin. d' \cos. l \sin. l' \end{array}}{\cos. d \cos. d' \cos. l \cos. l'} \right.,$$

et

$$\sin. m = \frac{\sin. a \cos. d' \cos. l' - \sin. a' \cos. d \cos. l - \sin. d \cos. d' \sin. l \cos. l' + \cos. d \sin. d' \cos. l \sin. l'}{2 \cos. d \cos. d' \cos. l \cos. l' \sin. \tfrac{1}{2} t}$$

Voici l'expression générale de l'horaire moyen m dans tous les cas du problème. Quand les observations ont été faites dans le même lieu on a $l = l'$, et en supposant la déclinaison constante dans l'intervalle $d = d'$, ce qui réduit la formule alors à $\sin. m = \frac{\sin. a - \sin. a'}{2 \cos. d \cos. l \sin. \frac{1}{2} t}$. Les circonstances dans la pratique sont presque toujours différentes; mais, l'intervalle n'étant que de quelques heures, la différence entre l et l' ne peut jamais être grande, et celle entre d et d' doit être encore moins considérable. Nous pourrons donc transformer la formule générale, en supposant ces différences très petites, pour déduire des expressions propres pour le calcul.

Faisons $l = l' + \Delta l$, et $d = d' + \Delta d$; et l'on aura

$$\cos. d' = \cos. d + \Delta d \sin. d$$
$$\sin. d' = \sin. d - \Delta d \cos. d$$
$$\cos. l' = \cos. l + \Delta l \sin. l$$
$$\sin. l' = \sin. l - \Delta l \cos. l$$

Substituons y ces expressions, en négligeant les produits des deux dimensions de Δl, Δd, et nous aurons

$$\sin. m = \left\{ \frac{\begin{array}{l} (\sin. a - \sin. a') \cos. d \cos. l + \Delta l (\sin. a \cos. d \sin. l - \sin. d \cos. d) \\ + \Delta d (\sin. a \sin. d \cos. l - \sin. l \cos. l) \end{array}}{2 \cos. d \cos. l \sin. \frac{1}{2} t (\cos. d \cos. l + \Delta l \cos. d \sin. l + \Delta d \sin. d \cos. l)} \right.$$

Représentons la latitude supposée du lieu où on observa la plus grande hauteur par l'', et faisons $l = l'' + \delta l''$, en suppo-

sant toujours que la différence $\delta l''$ est petite. Si l'on calcule un angle horaire moyen M avec cette latitude, on aura

$$\text{sin. M} = \frac{\text{sin. } a - \text{sin. } a'}{2 \text{ cos. } d \text{ cos. } l' \text{ sin. } \frac{1}{2} t} = \frac{\text{sin. } a - \text{sin. } a'}{2 \text{ cos. } d \text{ cos. } l \text{ sin. } \frac{1}{2} t + 2 \delta l'' \text{ cos. } d \text{ sin. } l \text{ sin. } \frac{1}{2} t}$$

Par conséquent, sin. m = sin. M + ...

$$\frac{\Delta l (\text{cos. } d \text{ sin. } l \text{ sin. } a' - \text{sin. } d \text{ cos. } d') + \Delta d (\text{sin. } d \text{ cos. } l \text{ sin. } a' - \text{sin. } l \text{ cos. } l') + \delta l'' \text{ cos. } d \text{ sin. } l (\text{sin. } a - \text{sin. } a')}{2 \text{ cos. } d \text{ cos. } l \text{ sin. } \frac{1}{2} t (\text{cos. } d \text{ cos. } l + \Delta l \text{ cos. } d \text{ sin. } l + \Delta d \text{ sin. } d \text{ cos. } l + \delta l'' \text{ cos. } d \text{ sin. } l)}$$

et, à très peu près,

$$m = \text{M} + \begin{cases} \dfrac{\Delta l (\text{sin. } l'' \text{ sin. } a' - \text{sin. } d)}{2 \text{ cos. } d \text{ cos.}^2 l'' \text{ cos. M sin. } \frac{1}{2} t} + \dfrac{\Delta d (\text{sin. } d \text{ sin. } a' - \text{sin. } l'')}{2 \text{ cos.}^2 d \text{ cos. } l'' \text{ cos. M sin. } \frac{1}{2} t} \\ + \dfrac{\delta l'' \text{ sin. } l'' (\text{sin. } a - \text{sin. } a')}{2 \text{ cos.}^2 d \text{ cos.}^2 l'' \text{ cos. M sin. } \frac{1}{2} t} \end{cases}$$

Substituant sin. $a' = \cos. b' \cos. d \cos. l'' + \sin. d \sin. l''$ dans le second et le troisième membre de la droite, et sin. $\text{M} = \frac{\text{sin. } a - \text{sin. } a'}{2 \text{cos. } d \text{ cos. } l' \text{ sin. } \frac{1}{2} t}$ dans le dernier, il résultera

$$m = \text{M} + \begin{cases} \dfrac{\Delta l (\text{cos } b' \text{ tan. } l'' - \text{tan. } d)}{2 \text{ cos. M sin. } \frac{1}{2} t} + \dfrac{\Delta d (\text{cos. } b' \text{ tan. } d - \text{tan. } l'')}{2 \text{ cos. M sin. } \frac{1}{2} t} \\ + \delta l'' \text{ tan. } l'' \text{ tan. M.} \end{cases}$$

Formule qui donne la valeur de l'horaire moyen pour le calcul relatif au lieu de la plus grande hauteur.

Si l'on suppose $l' = l + \Delta l$, et $d' = d + \Delta d$, on aura, en substituant comme nous avons fait auparavant,

$$\text{sin. } m = \begin{cases} (\text{sin. } a - \text{sin. } a') \text{ cos. } d' \text{ cos. } l' + \Delta l (\text{sin. } d' \text{ cos. } d' - \text{sin. } a' \text{ cos. } d' \text{ sin. } l') \\ + \Delta d (\text{sin. } l' \text{ cos. } l' - \text{sin. } d' \text{ sin. } d' \text{ cos. } l') \\ 2 \text{ cos. } d' \text{ cos. } l' \text{ sin. } \frac{1}{2} t (\text{cos. } d' \text{ cos. } l' + \Delta l \text{ cos. } d' \text{ sin. } l' + \Delta d \text{ sin. } d' \text{ cos. } l') \end{cases}$$

En représentant par l''' la latitude estimée du lieu où on a observé la plus petite hauteur, et en faisant $l''' + \delta l''' = l'$, et

sin. $\text{M}' = \frac{\text{sin. } a - \text{sin. } d}{2 \text{ cos. } d' \text{ cos. } l'' \text{ sin. } \frac{1}{2} t}$, ou, ce qui revient au même,

$$\text{sin. M} = \frac{\text{sin. } a - \text{sin. } a'}{2 \text{ cos. } d' \text{ cos. } l' \text{ sin. } \frac{1}{2} t + 2 \delta l'' \text{ cos. } d' \text{ sin. } l' \text{ sin. } \frac{1}{2} t}$$

on aura sin. m = sin. M' + ...

$$\frac{\Delta l (\text{sin. } d' \text{ cos. } d' - \text{sin. } a \text{ cos. } d' \text{ sin. } l') + \Delta d (\text{sin. } l' \text{ cos. } l' - \text{sin. } a \text{ sin. } d' \text{ cos. } l') + \delta l'' \text{ cos. } d' \text{ sin. } l' (\text{sin. } a - \text{sin. } a')}{2 \text{ cos. } d' \text{ cos. } l' \text{ sin. } \frac{1}{2} t (\Delta l \text{ cos. } d' \text{ sin. } l' + \Delta d \text{ sin. } d' \text{ cos. } l' + \delta l'' \text{ cos. } d' \text{ sin. } l')}$$

et, à très peu près,

$$m = M' + \begin{cases} \frac{\Delta l\ (\sin. d'' - \sin. P'' \sin. a)}{2 \cos. d' \cos.^2 P'' \cos. M' \sin. \frac{1}{2} t} + \frac{\Delta d\ (\sin. P'' - \sin. d' \sin. a)}{2 \cos.^2 d' \cos. P'' \cos. M' \sin. \frac{1}{2} t} \\ + \frac{\partial P'' \sin. P'' \ (\sin. a - \sin. d')}{2 \cos. d' \cos.^2 P'' \sin. \frac{1}{2} t} \end{cases}$$

Par où, en substituant $\sin. a = \cos. b \cos. d' \cos. l''' + \sin. d' \sin. l'''$, et $\sin. M' = \frac{\sin. a - \sin. d'}{2 \cos. d' \cos. P'' \sin. \frac{1}{2} t}$, il résulte

$$m = M' + \begin{cases} \frac{\Delta l\ (\tan. d'' - \cos. b \tan. P'')}{2 \cos. M' \sin. \frac{1}{2} t} + \frac{\Delta d\ (\tan. P'' - \cos. b \tan. d')}{2 \cos. M' \sin. \frac{1}{2} t} \\ + \partial l''' \tan. l''' \tan. M'. \end{cases}$$

Formule de l'horaire moyen pour le calcul relatif au lieu de la plus petite hauteur.

En considérant ces formules, on voit facilement la manière dont on doit procéder pour obtenir l'horaire moyen. De l'intervalle, et de la différence en longitude entre les lieux des observations, on déduira t. Avec cette quantité, et les données du problème, on trouvera M par l'expression $\frac{\sin. a - \sin. d'}{2 \cos. d' \cos. P'' \sin. \frac{1}{2} t}$, si l'on veut faire le calcul relativement au lieu de la plus grande hauteur; ou bien on trouvera M′ par l'expression $\frac{\sin. a - \sin. d'}{2 \cos. d' \cos. P'' \sin. \frac{1}{2} t}$ pour faire le calcul relativement au lieu de la plus petite hauteur. Après quoi, il faudra appliquer à M, ou M′, les équations convenables pour avoir m.

Les variations de la latitude, et de la déclinaison, étant connues par la nature du problème, on pourroit calculer par les expressions ci-dessus les équations qui en dérivent; mais l'horaire moyen résteroit toujours affecté de l'erreur qui dépend de $\partial l''$, ou $\partial l'''$, dont le dégagement n'est pas praticable jusqu'à la conclusion de la latitude. Il me paroit donc préférable de laisser toutes les corrections pour le dernier résultat.

Mr. DOUWES a employé la formule sin. $M = \frac{\text{sin. } a - \text{sin. } d'}{2 \text{ cos. } d \text{ cos. } l \text{ sin. } \frac{1}{2} t}$ pour sa méthode, et le Dr. PEMBERTON l'a mise sous la forme sin. $M = \frac{\cos. \frac{1}{2}(a+d) \sin. \frac{1}{2}(d'-a)}{\cos. d \cos. l \sin. \frac{1}{2} t}$, qui est propre pour le calcul par les logarithmes, sans le secours des sinus naturels.

Après avoir déterminé M, ou M′, on aura (en représentant le petit horaire approché par H, et le grand horaire approché par H′), $H = M - \frac{1}{2} t$, et $H' = M' + \frac{1}{2} t$.

Avec un horaire, et la hauteur et la déclinaison correspondantes, il seroit facile de calculer la latitude par les régles ordinaires de la Trigonométrie Sphérique, mais la solution du problème exigéroit alors des distinctions des cas qui la rendroient complexe, et que l'on doit éviter autant que possible. Nous chercherons, donc, des formules pour arriver au résultat par un procédé plus simple, et nous nous proposerons de déterminer la distance méridienne du soleil au zénith $d \sim l$; car cette distance une fois connue, la conclusion de la latitude est très facile.

Reprenons la formule $\cos. b = \frac{\text{sin. } a - \text{sin. } d \text{ sin. } l}{\cos. d \cos. l}$, et nous aurons cos. b cos. d cos. l + sin. d sin. l = sin. a, d'où (en substituant 1 − sin. v. b = cos. b), on déduit

cos. d cos. l + sin. d sin. l = sin. a + sin. v. b cos. d cos. l,

et par conséquent,

$$\cos. (d \sim l) = \text{sin. } a + \text{sin. v. } b \cos. d \cos. l$$

ou (en représentant par L la latitude qui résulte du calcul),*

$$\cos. (d \sim L) = \text{sin. } a + \text{sin. v. } H \cos. d \cos. l''.$$

* En substituant dans cos. b cos. d cos. l + sin. d sin. l = sin. a, l'expression cos. b = susin. v. b − 1, on déduiroit susin. v. b cos. d cos. l − cos. d cos. l + sin. d sin. l = sin. a, et par conséquent cos. $(d + l)$ = susin. v. b cos. d cos. l − sin. a. Je laisse pour une autre occasion le détail des applications qu'on pourroit faire de cette formule.

De cette équation on tire

$$1 - \text{cos.}\,(d \sim \text{L}) = 1 - \text{sin.}\,a - \text{sin. v. H cos.}\,d \,\text{cos.}\,l''$$

$$\text{sin. v.}\,(d \sim \text{L}) = \text{cos. v.}\,a - \text{sin. v. H cos.}\,d \,\text{cos.}\,l''$$

$$\text{sin. v.}\,(d \sim \text{L}) = \text{cos. v.}\,a \left(1 - \frac{\text{sin. v. H cos.}\,d \,\text{cos.}\,l''}{\text{cos. v.}\,a}\right).$$

Premiere formule, pour calculer la distance méridienne du soleil au zénith $d \sim \text{L}$, par les sinus-verses. En faisant donc $\frac{\text{sin. v. H cos.}\,d \,\text{cos.}\,l''}{\text{cos. v.}\,a} = \text{cos. N}$, on aura

$$\text{sin. v.}\,(d \sim \text{L}) = \text{cos. v.}\,a \,\text{sin. v. N.}$$

De l'équation $\text{cos.}\,(d \sim \text{L}) = \text{sin.}\,a + \text{sin. v. H cos.}\,d \,\text{cos.}\,l''$ on tire aussi

$$1 + \text{cos.}\,(d \sim \text{L}) = 1 + \text{sin.}\,a + \text{sin. v. H cos.}\,d \,\text{cos.}\,l''$$

$$\text{susin. v.}\,(d \sim \text{L}) = \text{sucos. v.}\,a + \text{sin. v. H cos.}\,d \,\text{cos.}\,l''$$

$$\text{susin. v.}\,(d \sim \text{L}) = \text{sucos. v.}\,a \left(1 + \frac{\text{sin. v. H cos.}\,d \,\text{cos.}\,l''}{\text{sucos. v.}\,a}\right)$$

Seconde formule, pour faire le calcul, par les sinus-verses. En faisant $\frac{\text{sin. v. H cos.}\,d \,\text{cos.}\,l''}{\text{sucos. v.}\,a} = \text{cos. N}$, on aura donc,

$$\text{susin. v.}\,(d \sim \text{L}) = \text{sucos. v.}\,a \,\text{susin. v. N.}$$

Comme l'arc $d \sim \text{L}$ est toujours moindre que 90°, ... sin. v. $(d \sim \text{L})$ sera sans exception plus petit que susin. v. $(d \sim \text{L})$; et, par conséquent, la première formule préférable à la seconde.

De la première formule, on tire

$$\text{sin.}^2 \tfrac{1}{2}\,(d \sim \text{L}) = \text{cos.}^2 \tfrac{1}{2}\,(90° + a) \left(1 - \frac{\text{sin.}^2 \frac{1}{2}\,\text{H cos.}\,d \,\text{cos.}\,l''}{\text{cos.}^2 \frac{1}{2}\,(90° + a)}\right)$$

et $\text{sin.}\,\tfrac{1}{2}\,(d \sim \text{L}) = \text{cos.}\,\tfrac{1}{2}\,(90° + a) \sqrt{1 - \frac{\text{sin.}^2 \frac{1}{2}\,\text{H cos.}\,d \,\text{cos.}\,l''}{\text{cos.}^2 \frac{1}{2}\,(90° + a)}}$

Troisième formule. Au moyen de la quelle on pourra calculer $d \sim \text{L}$ par les sinus; car en faisant $\frac{\text{sin.}\,\frac{1}{2}\,\text{H} \sqrt{\text{cos.}\,d \,\text{cos.}\,l''}}{\text{cos.}\,\frac{1}{2}\,(90° + a)} = \text{sin. N}$, on aura $\text{sin}\,\tfrac{1}{2}\,(d \sim \text{L}) = \text{cos.}\,\tfrac{1}{2}\,(90° + a)\,\text{cos. N.}$

De la seconde formule on tire

$$\cos.^2\tfrac{1}{2}(d\sim L) = \sin.^2\tfrac{1}{2}(90^\circ + a)\left(1 + \frac{\sin.^2\frac{1}{2}H\cos.d\cos.P}{\sin.^2\frac{1}{2}(90^\circ + a)}\right)$$

et $\cos.\frac{1}{2}(d\sim L) = \sin.\frac{1}{2}(90^\circ + a)\sqrt{1 + \frac{\sin.^2\frac{1}{2}H\cos.d\cos.P}{\sin.^2\frac{1}{2}(90^\circ + a)}}$

Quatrième formule. A l'aide de laquelle on pourra calculer $d \sim L$ par les sinus et les tangentes; car, en faisant $\frac{\sin.\frac{1}{2}H\sqrt{\cos.d\cos.P}}{\sin.\frac{1}{2}(90^\circ + a)} = \tan.N$, on aura $\cos.\frac{1}{2}(d\sim L) = \frac{\sin.\frac{1}{2}(90^\circ + a)}{\cos.N}$.

On doit remarquer que $\sin.\frac{1}{2}(d\sim L)$ est toujours moindre que $\cos.\frac{1}{2}(90^\circ + a)$, et que $\cos.\frac{1}{2}(d\sim L)$ est toujours plus grand que $\sin.\frac{1}{2}(90^\circ + a)$; ce qui rend la troisième formule plus exacte pour le calcul que la formule quatrième. Cependant, comme, en faisant usage des logarithmes sinus et tangentes seulement, le total des opérations est un peu plus court par le moyen de la dernière, on pourra préférer cette formule quand les tables qu'on emploie ne contiendront pas les sécantes.

Voici une autre manière de conclure la latitude, après avoir déterminé l'angle horaire; car, au lieu de la distance méridienne du soleil au zénith, on pourroit calculer la différence entre cette distance, et la distance au zénith correspondante à l'observation près du midi, ou ce qui revient au même, la différence entre la hauteur méridienne, et la plus grande hauteur observée. La formule $\cos.(d\sim l) = \sin.a + \sin.v.b\cos.d\cos.l$, donne

$$\cos.(d\sim l) - \cos.(90^\circ - a) = \sin.v.b\cos.d\cos.l,$$

et par conséquent

$$2\sin.\tfrac{1}{2}\left(90^\circ - a + (d\sim l)\right)\sin.\tfrac{1}{2}\left(90^\circ - a - (d\sim l)\right) = \sin.v.b\cos.d\cos.l$$

d'où l'on déduit

$$\sin.\tfrac{1}{2}\left(90^\circ - a - (d\sim l)\right) = \cos.\tfrac{1}{2}\left(90^\circ + a + (d\sim l)\right)$$

$$= \frac{\sin.v.b\cos.d\cos.l}{2\sin.\frac{1}{2}\left(90^\circ - a + (d\sim l)\right)} = \frac{\sin.^2\frac{1}{2}b\cos.d\cos.l}{\sin.\frac{1}{2}\left(90^\circ - a + (d\sim l)\right)}$$

ou

$$\sin.\ \tfrac{1}{2}\left(90^\circ - a - (d \sim l)\right) = \cos.\ \tfrac{1}{2}\left(90^\circ + a + (d \sim l)\right)$$

$$= \frac{\text{sin. v. } b \cos.\ d \cos.\ l}{2 \cos.\ \frac{1}{2}\left(90^\circ + a - (d \sim l)\right)} = \frac{\text{sin.}^2\ \frac{1}{2}\, b \cos.\ d \cos.\ l}{\cos.\ \frac{1}{2}\left(90^\circ + a - (d \sim l)\right)}$$

Après avoir trouvé $90^\circ + a + (d \sim l)$, on déduiroit facilement la distance méridienne $d \sim l$. Avec cette formule, on épargneroit quelques logarithmes, mais l'ensemble des opérations ne seroit pas pour cela plus facile. Je crois donc avantageux de préférer l'expression qui donne directement $d \sim l$, et je supposerai qu'on fasse toujours le calcul par cette méthode.

Si l'on reprend l'équation cos. b' cos. d' cos. $l' =$ . . . . . sin. $a' -$ sin. d' sin. l', on aura, comme auparavant, cos. $(d' \sim l') =$ sin. $a' +$ sin. v. b' cos. d' cos. l', ou (en représentant par L' la latitude calculée du lieu de la plus petite hauteur), cos. $(d' \sim L') =$ sin. $a' +$ sin. v. H′ cos. d' cos. l''. En suivant le procédé ci-dessus, on déduira d'ici quatre formules pour calculer la distance méridienne $d' \sim L'$, relative au lieu de la plus petite hauteur; formules qui sont analogues à celles que nous avons établies pour $d \sim L$ relativement au lieu de la plus grande hauteur.

Mais, le calcul précédent étant fait avec des élémens qui ne sont pas rigoureusement vrais, il faut à présent chercher des moyens pour porter le résultat de la méthode jusqu'au degré d'exactitude qui est nécessaire dans la pratique de la Navigation.

Considérons d'abord le calcul relativement au lieu de la plus grande hauteur.

L'expression employée est

$$\text{cos. } (d \sim L) = \text{sin. } a + \text{sin. v. H cos. } d \text{ cos. } l'',$$

où l'' représente la latitude estimée, et H le petit horaire déduit du calcul. Les erreurs de ces quantités seront toujours petites. On pourra donc avoir recours au calcul différentiel pour déterminer leur influence, et l'on aura

$$-\delta(d\sim L)\sin.(d\sim L)=\begin{cases} d\,H\sin.H\cos.d\cos.l'' \\ -\delta l''\sin.v.H\cos.d\sin.l'' \end{cases}$$

et

$$-\delta(d\sim L)\sin.(d\sim L)=\begin{cases} d\ H.\ \sin.H\cos.d\cos.l''+ \\ \delta l''\cos.H\cos.d\sin.l''-\delta l''\cos.d.\sin.l'' \end{cases}$$

Mais nous avons trouvé

$$\delta H=\delta M=\begin{cases} \frac{\Delta l\,(\cos.b'\tan.P-\tan.d)}{2\cos.M\sin.\frac{1}{2}t}+\frac{\Delta d\,(\cos.b'\tan.d-\tan.P)}{2\cos.M\sin.\frac{1}{2}t} \\ +\delta l''\tan.l''\tan.M \end{cases}$$

ou, ce qui revient au même,

$$\delta H=\delta M=\begin{cases} \frac{\Delta l\,(\cos.H'\tan.P-\tan.d)}{2\cos.M\sin.\frac{1}{2}t}+\frac{\Delta d\,(\cos.H'\tan.d-\tan.P)}{2\cos.M.\sin.\frac{1}{2}t} \\ +\delta l''\tan.l''\tan.M. \end{cases}$$

Donc, en substituant, et en prenant l'' pour L (car ces quantités ne diffèrent que de peu de chose), il résultera

$$\delta(d\sim L)=\begin{cases} \frac{\Delta l\sin.H.\cos.d\cos.P\,(\tan.d-\cos.H'\tan.P)}{2\cos.M.\sin.\frac{1}{2}t\sin.(d\sim P)}+\frac{\Delta d\sin.H\cos.d\cos.P\,(\tan.P-\cos.H'\tan.d)}{2\cos.M\sin.\frac{1}{2}t\sin.(d\sim P)} \\ +\frac{\delta P\cos.d\sin.P\,(\cos.M-\cos.\frac{1}{2}t)}{\cos.M\sin.(d\sim P)} \end{cases}$$

$$\delta(d\sim L)=\begin{cases} \frac{\Delta l\sin.H\,(\tan.d-\cos.H'\tan.P)}{2\cos.M\sin.\frac{1}{2}t\,(\tan.d\sim\tan.P)}+\frac{\Delta d\sin.H\,(\tan.P-\cos.H'\tan.d)}{2\cos.M\sin.\frac{1}{2}t\,(\tan.d\sim\tan.P)} \\ +\frac{\delta P\,(\cos.M-\cos.\frac{1}{2}t)}{\cos.M\,(\tan.d\cot.P\sim 1)} \end{cases}$$

$$\delta(d\sim L)=\begin{cases} \frac{\Delta l\sin.H\,(\tan.d\cot.P-\cos.H')}{(\sin.H'-\sin.H)\,(\tan.d\cot.P\sim 1)}+\frac{\Delta d\sin.H\,(\cot.d\tan.P-\cos.H')}{(\sin.H'-\sin.H)\,(\cot.d\tan.P\sim 1)} \\ +\frac{\delta P\,(\cos.M-\cos.\frac{1}{2}t)}{\cos.M\,(\tan.d\cot.P\sim 1)} \end{cases}$$

Voilà les corrections qu'on doit appliquer à la distance méridienne du soleil au zénith $d\sim L$. Les mêmes corrections ont lieu aussi pour la latitude calculée L; car $=\delta L=\delta(d\sim L)$. Le signe supérieur, quand le soleil passe par le quart de méridien où se trouve le pole élevé, le signe inférieur dans les autres cas.

A l'aide des expressions ci-dessus, on pourroit former des

tables pour avoir facilement les corrections relatives aux variations Δl, Δd; ce qui seroit convenable pour rendre la méthode générale, et très exacte.

A l'égard de la correction relative à $d l''$, voici le procédé qui me paroit le plus simple, et le plus expéditif, et par conséquent le plus avantageux pour la pratique. On peut faire le calcul tant pour une latitude supposée l''', que pour une autre latitude l', de manière que la différence entre l''', et l'' soit peu considérable. Ainsi l'on aura (en représentant la latitude calculée résultante de l'' par L, et la latitude calculée résultante de l''' par L') $\mp \partial L = \frac{\partial l'' (\cos. M - \cos. \frac{1}{2} l)}{\cos. M (\tan. d \cot. l'' \sim 1)}$, et à très peu près

$$\mp \partial L' = \frac{\partial l'' (\cos. M - \cos. \frac{1}{2} l)}{\cos. M (\tan. d \cot. l'' \sim 1)}$$

De là on tire $\partial L : \partial L' :: \partial l'' : \partial l'''$

par conséquent $(\partial L \simeq \partial l'') : \partial L :: (\partial L' \simeq \partial l'') : \partial L'$

et $(\partial L \simeq \partial l'') \simeq (\partial L' \simeq \partial l''') : (\partial L \simeq \partial l'') :: (\partial L \simeq \partial L') : \partial L$

d'où il résulte $\partial L = \frac{(\partial L \simeq \partial l'')(\partial L \simeq \partial L')}{(\partial L \simeq \partial l'') \simeq (\partial L' \simeq \partial l'')}$

c'est-à dire $\partial L = \frac{(L \sim l'')(L \sim L')}{(L' \sim l'') \simeq (L' \sim l''')}$.

Expression de la correction qu'on doit appliquer à la latitude calculée L. Le signe supérieur, quand les deux latitudes calculées s'éloignent dans le même sens des respectives latitudes supposées; le signe inférieur, dans le cas contraire.

On pourroit aussi déduire la correction qu'on doit appliquer à la latitude supposée, et l'on auroit $\partial l'' = \frac{(L \sim l'')(l'' \sim l''')}{(L \sim l'') \simeq (L \sim l''')}$.

La manière d'appliquer la correction ∂L, ou celle $\partial l''$, est évidente, si l'on fait attention que la latitude vraie doit être comprise entre les deux latitudes supposées, ou entre les deux latitudes calculées, dans tous les cas, excepté celui où les deux

latitudes calculées s'éloignent dans le même sens des correspondantes latitudes supposées; et que dans cette circonstance la latitude vraie se trouve près de la latitude supposée (ou calculée) qui diffère le moins de sa correspondante latitude calculée (ou supposée).

Pour le calcul relativement au lieu de la petite hauteur, on déduiroit aussi, par un procédé semblable,

$$\mp \partial L' = \partial (d' \sim L') = \begin{cases} \frac{\Delta l \sin. H' (\cos. H - \tan. d' \cot. l'')}{(\sin. H' - \sin. H)(\tan. d' \cot. l'' \sim 1)} + \frac{\Delta d \sin. H' (\cos. H - \cot. d' \tan. l'')}{(\sin. H' - \sin. H)(\cot. d' \tan. l'' \sim 1)} \\ + \frac{\partial l'' (\cos. M' - \cos. \frac{1}{2} l)}{\cos. M' (\tan. d' \cot. l'' \sim 1)} \end{cases}$$

Expressions auxquelles on peut appliquer ce qui vient d'être dit au sujet des formules analogues que nous avons trouvé pour le lieu de la grande hauteur.

Après avoir établi les formules nécessaires pour calculer la latitude, nous considérerons les erreurs qui peuvent influer dans le résultat, pour déterminer les circonstances favorables à l'usage du problème. Nous examinerons aussi, s'il est indifférent de faire le calcul relativement au lieu de la grande hauteur, ou relativement au lieu de la petite hauteur, ou laquelle de ces deux manières d'opérer est la préférable.

Pour la plus grande facilité des comparaisons, nous représenterons par L la latitude calculée relativement à la grande hauteur, ou à la petite hauteur, et nous employerons les dénominations des élémens vrais, en prenant aussi indistinctement $\partial l''$, ou $\partial l'''$.

L'équation générale qui exprime la rélation entre une erreur commise dans la latitude supposée, et l'erreur résultante dans la latitude calculée, est $\mp \partial L = \frac{\partial l'' (\cos. m - \cos. \frac{1}{2} l)}{\cos. m (\tan. d \cot. l \sim 1)}$; ce qui prouve que l'erreur de la latitude calculée n'est pas fort différente, soit qu'on calcule pour le lieu de la grande hauteur, ou de la petite hauteur.

Comme m est plus grand ou plus petit que $\frac{1}{2}t$, selon qu'on a fait les observations du même coté du méridien, ou l'une avant et l'autre après midi, on voit 1°. Que, dans le cas où les observations sont de la même espèce, les erreurs de la latitude supposée, et de la latitude calculée ont le même signe, quand le soleil passe par le quart du méridien où se trouve le pole élevé; et que ces erreurs ont des signes contraires, dans toutes les autres circonstances. 2°. Que la régle inverse a lieu, quand les observations sont de différente espèce.

Supposons qu'on ait commis une petite erreur ∂t dans l'intervalle. On aura $\partial m - \frac{1}{2}\partial t = \partial b$, et $\partial m + \frac{1}{2}\partial t = \partial b'$; et en reprenant sin. $m = \frac{\text{sin. } a - \text{sin. } d'}{2 \text{ sin. } \frac{1}{2}t \text{ cos. } d \text{ cos. } l}$, et différentiant, $\partial m = \ldots - \frac{1}{2}\partial t \text{ cot. } \frac{1}{2}t \text{ tan. } m$. Ainsi $\partial b = -\frac{1}{2}\partial t \text{ tan. } m \text{ cot. } \frac{1}{2}t - \frac{1}{2}\partial t$ et $\partial b' = -\frac{1}{2}\partial t \text{ tan. } m \text{ cot. } \frac{1}{2}t + \frac{1}{2}\partial t$.

En différentiant l'équation

$$\text{cos. } (d \sim \text{L}) = \text{sin. } a + \text{sin. v. } b \text{ cos. } d \text{ cos. } l$$

on aura

$$\mp \partial \text{L} = -\frac{\partial b \text{ sin. } b \text{ cos. } d \text{ cos. } l}{\text{sin. } (d \sim l)}$$

ce qui, en substituant la valeur de ∂b ci-dessus, donne

$$\mp \partial \text{L} = \frac{\frac{1}{2}\partial t \text{ sin. } b \text{ cos. } d \text{ cos. } l \, (\text{tan. } m \text{ cot. } \frac{1}{2}t + 1)}{\text{sin. } (d \sim l)}$$

$$\mp \partial \text{L} = \frac{\frac{1}{2}\partial t \text{ sin. } b \text{ sin. } b' \text{ cos. } d \text{ cos. } l}{\text{cos. } m \text{ sin. } \frac{1}{2}t \text{ sin. } (d \sim l)}$$

$$\mp \partial \text{L} = \frac{\partial t \text{ sin } b \text{ sin. } b' \text{ cos. } d \text{ cos. } l}{(\text{sin. } b' - \text{sin } b) \text{ sin. } (d \sim l)}$$

$$\mp \partial \text{L} = \frac{\partial t \text{ sin. } b \text{ sin. } b'}{(\text{sin } b' - \text{sin. } b)(\text{tan. } d \sim \text{tan. } l)}$$

$$\mp \partial \text{L} = \frac{\partial t}{(\text{cosec. } b - \text{cosec. } b')(\text{tan. } d \sim \text{tan. } l)}$$

Expression de l'influence de l'erreur de l'intervalle, en calculant par la grande hauteur.

En différentiant l'équation

$$\cos.(d \sim \mathrm{L}) = \sin. a' + \text{sin. v.}\, b' \cos. d \cos. l$$

on aura

$$\mp \partial \mathrm{L} = -\partial b' \sin. b' \cos. d \cos. l\ ;$$

ce qui, en substituant la valeur de $\partial b'$ ci-dessus, donnera les mêmes expressions qu'on vient de trouver pour $\mp \partial \mathrm{L}$.

On voit donc, que l'influence d'une erreur commise dans l'intervalle est la même dans les deux manières de faire le calcul.

De la formule qui exprime l'influence de l'erreur de la latitude supposée, on déduit

1°. Que, l'erreur de la latitude calculée est nulle quand une des hauteurs observées est la hauteur méridienne. Ainsi, il convient de faire une observation près du midi.

2°. Que, les distances au méridien étant égales, dans les deux cas, l'erreur du résultat sera plus petite si les deux observations sont de différente espèce, que si elles étoient de la même espèce.

3°. Qu'en supposant l'horaire moyen constant, il convient d'augmenter l'intervalle, quand les observations sont de la même espèce, et le diminuer quand les observations sont de différente espèce.

4°. Qu'en supposant un horaire constant, il convient toujours de diminuer l'autre horaire.

5°. Que, les circonstances les moins favorables pour l'usage de la méthode sont celles, où le soleil passe par le zénith, ou près du zénith.

De la formule qui exprime l'influence de l'erreur de l'intervalle ∂t, on déduit les mêmes conséquences, à l'exception d'une circonstance particulière de la quatrième ; car dans le cas des observations de la même espèce, et en supposant le petit horaire constant, il conviendroit sous ce rapport d'augmenter le grand horaire pour diminuer l'erreur de la latitude calculée.

On doit cependant remarquer que, quoique, en augmentant l'intervalle, l'on diminue l'influence d'une erreur supposée dans cet élément, par un effet de cette même augmentation, on augmente aussi la probabilité de commettre une erreur plus considérable dans la mesure du tems écoulé. Il me paroit, donc, toutes considérations faites, qu'on peut adopter les régles précédentes généralement.

Voyons à présent quelle est l'influence des erreurs qu'on peut commettre dans les hauteurs du soleil.

En différentiant $\sin.\ m = \frac{\sin.\ a - \sin.\ a'}{2\sin.\ \frac{1}{2}\,l\cos.\ d\cos.\ l}$, on aura $\partial m = \frac{\partial a\cos.\ a}{2\cos.\ m\sin.\ \frac{1}{2}\,l\cos.\ d\cos.\ l}$; et par conséquent $\partial b = \frac{\partial a\cos.\ a}{2\cos.\ m\sin.\ \frac{1}{2}\,l\cos.\ d\cos.\ l}$

ou $\partial b = \frac{\partial a\cos.\ a}{(\sin.\ b' - \sin.\ b)\cos.\ d\cos.\ l}$; et $\partial b' = \frac{\partial a\cos.\ a}{2\cos.\ m\sin.\ \frac{1}{2}\,l\cos.\ d\cos.\ l}$,

ou $\partial b' = \frac{\partial a\cos.\ a}{(\sin.\ b' - \sin.\ b)\cos.\ d\cos.\ l}$

En différentiant l'équation

$$\cos.\ (d \sim \mathrm{L}) = \sin.\ a + \sin.\mathrm{v}.\ b\cos.\ d\cos.\ l,$$

on aura $\mp\partial\mathrm{L} = -\frac{\partial a\cos.\ a + \partial b\sin.\ b\cos.\ d\cos.\ l}{\sin.\ (d \sim l)}$;

ce qui, en substituant la valeur de b trouvée ci-dessus, donne $\mp\partial\mathrm{L} = -\frac{\partial a\cos.\ a\sin.\ b'}{(\sin.\ b' - \sin.\ b)\sin.\ (d \sim l)}$. Expression de l'erreur résultante de l'erreur commise dans la grande hauteur, en faisant le calcul relativement à cette hauteur.

En prenant l'équation

$$\cos.\ (d \sim \mathrm{L}) = \sin.\ a' + \sin.\mathrm{v}.\ b'\cos.\ d\cos.\ l$$

on aura $\mp\partial\mathrm{L} = -\frac{\partial b'\sin.\ b'\cos.\ d\cos.\ l}{\sin.\ (d \sim l)}$,

et par conséquent $\mp\partial\mathrm{L} = -\frac{\partial a\cos.\ a\sin.\ b'}{(\sin.\ b' - \sin.\ b)\sin.\ (d \sim l)}$.

Expression de l'influence d'une erreur commise dans la grande hauteur, en calculant par la petite hauteur.

L'influence d'une erreur ∂a est donc la même dans les deux manières de faire le calcul.

Si l'on suppose une erreur $\partial a'$ dans la petite hauteur, on trouvera aussi, en suivant le même procédé, $\mp \partial L = \frac{\partial a' \cos. a' \sin. b}{(\sin. b' - \sin. b) \sin. (d - l)}$ pour l'expression de l'erreur du résultat, soit qu'on fasse le calcul par la grande hauteur, ou par la petite hauteur.

En supposant la même erreur dans les deux hauteurs, on voit que l'erreur résultante de la grande hauteur, est à l'erreur résultante de la petite hauteur, comme cos. a sin. b', à cos. a' sin. b, ou (parceque nous avons représenté par e l'azimuth correspondant à a, et par e' l'azimuth correspondant à a', et considérant que $\frac{\cos. d \sin. b}{\sin. e} = \cos. a$ et $\frac{\cos. d \sin. b'}{\sin. e'} = \cos. a'$) comme sin. e', à sin. e. Ainsi, l'influence d'une erreur supposée dans les deux hauteurs sera en raison inverse des sinus des azimuths.

La formule $\sin. M = \frac{\cos. \frac{1}{2}(a + a') \sin. \frac{1}{2}(a - a')}{\sin. \frac{1}{2} t \cos. d \cos. l}$ est une équation de condition, qui suppose que la déclinaison du soleil et la latitude géographique sont les mêmes pour les deux observations. Nous avons donné des formules pour corriger le résultat des erreurs qui dérivent de cette fausse supposition dans tous les cas du problème; et ces corrections pourront se trouver facilement à l'aide des expressions établies réduites en tables. Au défaut de ces moyens, on pourra réduire une des hauteurs à celle qu'on auroit observé dans le lieu où l'on a observé l'autre, comme on le pratique ordinairement dans la méthode de Douwes. Mais, quoique l'identité des deux latitudes ait lieu alors, on n'evite pas pour cela l'erreur résultante du changement en déclinaison. Il s'agit à présent d'examiner l'influence de chacune de ces causes.

La correction qu'on doit appliquer à la distance méridienne, ou à la latitude calculée, en raison de la variation de la latitude est $= \frac{\Delta l \sin. b (\tan. d \cot. l - \cos. b')}{(\sin. b' - \sin. b)(\tan. d \cot. l \sim 1)}$, en calculant par la grande hauteur. Par conséquent, l'erreur qu'on commettra, en négligeant cette correction, sera nulle, ou négligeable, quand on aura fait une observation à midi, ou très près du midi.

La même erreur sera aussi nulle, quand l'observation de la petite hauteur aura été faite dans le premier vertical, car alors $\tan. d \cot. l = \cos. b'$.

En faisant le calcul par la petite hauteur la correction qu'on doit appliquer est $= \frac{\Delta l \sin. b' (\cos. b - \tan. d \cot. l)}{(\sin. b' - \sin. b)(\tan. d \cot. l \sim 1)}$. L'erreur qu'on commettra, en la négligeant, ne sera donc pas nulle dans les circonstances générales du problème; car b' aura ordinairement une valeur considérable, et l'égalité $\cos. b = \tan. d \cot. l$ n'aura pas lieu quand on fera l'observation de la grande hauteur près du midi.

L'erreur qui résulte de négliger la variation de la déclinaison est $= \frac{\Delta d \sin. b (\cot. d \tan. l - \cos. b')}{(\sin. b' - \sin. b)(\cot. d \tan. l \sim 1)}$, en calculant par la grande hauteur; et cette erreur sera nulle, ou négligeable, quand une des observations aura été faite à midi, ou près du midi.

La même erreur deviendra aussi nulle quand l'angle parallactique, ou de variation, à l'instant de l'observation de la petite hauteur, sera droit; car alors $\cot. d \tan. l = \cos. b'$.

L'erreur du résultat, en calculant par la petite hauteur, est $= \frac{\Delta d \sin. b' (\cos. b - \cot. d \tan. l)}{(\sin. b' - \sin. b)(\cot. d \tan. l \sim 1)}$. Par où l'on voit, que cette erreur sera plus grande que la précédente dans les circonstances générales du problème.

Ces réflexions rendent préférable le calcul, relativement à

la grande hauteur. Elles prouvent aussi, que, quand on aura pris une hauteur près du midi (ce qu'il convient de faire dans tous les cas possibles), on pourra se dispenser de réduire l'une des deux hauteurs à celle qu'on auroit observé dans le lieu où l'on observa l'autre.

Par la même raison, quand on employera la méthode de corriger une des hauteurs, on pourra établir comme principe général, qu'on réduise la petite hauteur à celle qui conviendroit au lieu où l'on a observé l'autre; car les circonstances qui pourroient le modifier ne sont pas assez importantes pour passer par l'inconvénient de compliquer avec des exceptions les règles de la pratique. Cependant, pour examiner toutes les circonstances de cette solution du problème, nous déterminerons les erreurs qui résultent dans la hauteur réduite, des erreurs qui peuvent affecter les élémens qu'on emploie dans la réduction.

Supposons la distance directe entre les lieux des deux observations $= n$, l'angle formé par l'azimuth du soleil et l'aire de vent qui conduit du lieu de la grande hauteur au lieu de l'autre $= r$, et l'angle formé par l'azimuth et l'aire de vent dans le lieu de la petite hauteur $= r'$. On aura $n \cos. r$ pour la réduction de la grande hauteur, et $n \cos. r'$ pour la réduction de la petite hauteur.

En supposant une certaine erreur dans la mesure de l'aire de vent, on aura pour les erreurs résultantes ∂a, $\partial a'$ dans les hauteurs, $\partial a = -\partial r\, n \sin. r$, et $\partial a' = -\partial r'\, n \sin. r'$. Mais (en représentant l'erreur de la latitude calculée par $\partial L'$, quand on opère relativement à la petite hauteur), on a trouvé ci-devant $\partial L = \frac{\partial a \cos. a \sin. b'}{(\sin. b' - \sin. b) \sin. (d \sim l)}$, et $\partial L' = \frac{\partial a' \cos. a' \sin. b}{(\sin. b' - \sin. b) \sin. (d \sim l)}$.

Donc, en substituant les expressions précédentes, on déduira
$\partial L : \partial L' : : \partial r\, n$ sin. r cos. a sin. $b' : \partial r'\, n$ sin r' cos. a' sin. b
et (parceque l'on suppose $\partial r = \partial r'$)
$\partial L : \partial L' : :$ sin. r sin. $e' :$ sin. r' sin. e.

Les erreurs qu'on doit craindre de l'usage du Compas dans les observations des azimuths sont comme les tangentes des hauteurs du soleil (voyez le Mémoire de M. BOUGUER, sur les meilleurs moyens d'observer en mer la déclinaison magnétique; *Prix de l'Académie des Sciences de Paris pour* 1781). Faisons donc, pour ce cas, $\partial r =$ B tan. a, et $\partial r' =$ B tan. a'. Par conséquent $\partial a = \partial r\, n$ sin. $r = n$ B tan. a sin r, et $\partial a' = \partial r' n$ sin. $r' = n$ B tan. a' sin. r'; et, en substituant ces expressions dans les formules ci-dessus, on déduira
$\partial L : \partial L' : : n$ B tan. a cos. a sin. r sin. $b' : n$ B tan. a' cos. a' sin. r' sin. b
et $\partial L : \partial L' : :$ sin. a sin. r sin. $b' :$ sin. a' sin. r' sin. b.

Pour déterminer l'influence d'une erreur commise dans la distance directe, on a $\partial a = \partial n$ cos. r, et $\partial a' = \partial n$ cos. r'; et par conséquent, en substituant dans les formules ci-dessus,
$\partial L : \partial L : : \partial n$ cos. r cos. a sin. $b' : \partial n$ cos. r' cos. a' sin. b
c'est-à-dire $\partial L : \partial L' : :$ cos. r sin. $e' :$ cos. r' sin. e.

Il convient ici de faire une réflexion, par laquelle je terminerai cet article. Les formules que nous avons trouvé pour exprimer l'influence des erreurs sont relatives au résultat qu'on obtient par le calcul d'une latitude supposée. Mais, quand par la méthode ci-dessus, ou par la répétition du calcul, ou par quelque autre procédé, on procure l'identité de la latitude supposée et de la latitude calculée, le cas est différent, et les équations établies ne sauroient donner la valeur exacte de l'erreur du résultat. Si une des données du problème est fausse, on sent, que par la nature de ces espèces de méthodes, il faudra

employer aussi une latitude fausse pour compenser cet effet, et la faire convenir avec la latitude calculée. Généralement parlant, on pourroit dire que quand les circonstances seront favorables pour diminuer l'influence de l'erreur de la donnée, l'erreur dans la latitude supposée nécessaire pour produire l'identité sera aussi moins considérable; mais l'expression juste ne sera pas celle que nous avons déduite. Pour trouver les formules qui convienent alors, on devroit suivre un autre procédé, dont je vais donner un exemple, en considérant l'erreur de l'intervalle.

L'erreur de la latitude calculée composée de celles qu'on peut attribuer à l'intervalle, et à la latitude supposée, est

$$\mp \partial L = \frac{\partial P\,(\cos. m - \cos. \frac{1}{2} t)}{\cos. m\,(\tan. d \cot. l \sim 1)} + \frac{\partial t \sin. b \sin. b'}{2 \cos m \sin. \frac{1}{2} t\,(\tan. d \sim \tan. l)}.$$

Mais, pour faire convenir la latitude calculée avec la latitude supposée, il faut que ∂L soit $= \partial l''$, donc

$$\mp \partial l'' = \frac{\partial P \tan. l\,(\cos. m - \cos. \frac{1}{2} t)}{\cos. m\,(\tan. d \sim \tan. l)} + \frac{\partial t \sin. b \sin. b'}{2 \cos. m \sin. \frac{1}{2} t\,(\tan. d \sim \tan. l)}.$$

Par conséquent,

$$\mp 2 \partial P \cos. m \sin. \tfrac{1}{2} t\,(\tan. d \sim \tan. l) - 2 \partial P \tan. l \sin. \tfrac{1}{2} t\,(\cos. m - \cos. \tfrac{1}{2} t) = \partial t \sin. b \sin. b'$$

c'est-à-dire

$$2 \partial P \cos. m \sin. \tfrac{1}{2} t\,(\tan. l - \tan. d) - 2 \partial P \tan. l \sin. \tfrac{1}{2} t\,(\cos. m - \cos. \tfrac{1}{2} t) = \sin. b \sin. b'$$

d'où l'on déduit $\partial l'' = \dfrac{\partial t \sin. b. \sin. b'}{\tan. l \sin. t - 2 \tan. d \cos. m \sin. \frac{1}{2} t}$

ou $\partial l'' = \dfrac{\partial t \sin. b \sin. b'}{\tan. l\,(\sin. b' \cos. b - \cos. b' \sin. b) - \tan. d\,(\sin. b' - \sin. b)}$

et finalement $\partial l'' = \dfrac{\partial t}{\tan. l\,(\cot. b - \cot. b') - \tan. d\,(\operatorname{cosec} b - \operatorname{cosec} b')}$

Expressions égales à celles qu'on trouve pour la méthode directe.

Nous remarquerons, au reste, que les équations relatives à Δd, et Δl, qui sont celles qu'on doit employer d'une manière absolue, pourront être appliquées immédiatement au résultat du calcul fait par chaque supposition séparément.

Méthode indirecte, en déduisant premièrement le plus grand Angle horaire.

Avec la latitude estimée, et les autres données relatives au lieu de la petite hauteur, on calculera le grand angle horaire (que nous représenterons par H′), par l'une des formules suivantes (Voyez ci-après la démonstration de ces formules),

$$\text{sin. v. } H' = \frac{2\cos.\frac{1}{2}(D'+l''+d')\sin.\frac{1}{2}(D+l''-d)}{\cos. l''\sin. D'}$$

$$\text{sin. v. } H' = \frac{\sqrt{\text{susin. v. }(D'+l''+d')\text{ sin. v. }(D'+l''-d)}}{\cos. l''\sin. D'}$$

$$\sin.\tfrac{1}{2}H' = \sqrt{\frac{\cos.\frac{1}{2}(D'+l''+d')\sin.\frac{1}{2}(D'+l''-d)}{\cos. l''\sin. D'}}$$

ou par toute autre formule de celles que fournit la Trigonométrie Sphérique pour le calcul de l'angle horaire. Et l'on déduira le petit angle horaire $H = H' \sim t$.

Après avoir déterminé le petit horaire, on conclura la distance méridienne du soleil au zénith, et la latitude, par le moyen des formules que nous avons établi pour la même opération dans la méthode précédente, en employant les données relatives au lieu de la grande hauteur.

On pourra aussi trouver la latitude exacte, en faisant le calcul de cette méthode avec des latitudes supposées un peu différentes des latitudes estimées, imitant le procédé que nous avons expliqué ci-dessus.

Après avoir considéré avec tant de détail la méthode qui précéde, nous ne ferons qu'indiquer les formules qu'on pourra tirer des équations fondamentales de celle que nous avons à présent sous les yeux, en y ajoutant seulement quelques réflexions générales.

On aura, pour exprimer la relation entre l'erreur de la latitude estimée et l'erreur résultante dans la latitude calculée,

$$\mp \delta L = \frac{\delta P \,(\sin. H - \sin. t - \tan. d \cot. P \sin. H)}{\sin. H' (\tan. d \cot. P \sim 1)}$$

$$\mp \delta L = \frac{\delta P \left(2 \sin. \tfrac{1}{2} H \cos. \tfrac{1}{2} (H' + t) - \tan. d \cot. P \sin. H\right)}{\sin. H' (\tan. d \cot. P \sim 1)}$$

On voit par cette expression, que l'erreur du résultat est nulle, ou très petite, quand on observe la grande hauteur à midi, ou près du midi.

De la même formule on peut déduire les circonstances qui sont avantageuses pour que la latitude calculée s'approche de la latitude vraie; mais je ne m'arrêterai pas, à présent, à les énoncer particulièrement.

Si l'on suppose une erreur δt dans l'intervalle, on trouvera que l'erreur résultante dans la latitude calculée est $\mp \delta L = \frac{\delta t \sin. b}{\tan. d \sim \tan. l}$. Cette erreur sera donc nulle, quand on a observé une hauteur à midi. Et l'influence d'une erreur supposée dans l'intervalle sera la même, quels que soient l'intervalle, et la distance à midi de l'observation de la petite hauteur.

L'erreur résultante d'une erreur δa supposée dans la grande hauteur est $\mp \delta L = -\frac{\delta a \cos. a}{\sin. (d \sim l)}$.

Et l'erreur résultante d'une erreur $\delta a'$ supposée dans la petite hauteur

$$\mp \delta L = \frac{\delta a' \cos. a' \sin. b}{\sin. b \sin. (d \sim l)}.$$

Si l'on fait le calcul de la distance méridienne avec la latitude du lieu où l'on a observé la petite hauteur, au lieu d'employer la latitude correspondante à l'autre hauteur, on commettra une erreur $\mp \delta L = \frac{\Delta l \sin. v. b \sin. l \cos. d}{\sin. (d \sim l)} = \frac{\Delta l \sin. v. b}{\tan. d \cot. l \sim 1}$.

Et si l'on fait le calcul de la distance méridienne avec la déclinaison correspondante à la petite hauteur, on commettra une

erreur $\mp \partial L = \frac{\Delta d \text{ sin. v. } b \text{ cos. } l \text{ sin. } d}{\text{sin. } (d \sim l)} = \frac{\Delta d \text{ sin. v. } b}{\text{cot. } d \text{ tan. } l \sim 1}$.

Quand on aura pris une hauteur près du midi, on pourra donc faire tout le calcul, en employant la latitude et la déclinaison correspondantes à la petite hauteur; et le résultat donnera avec assez d'exactitude la latitude du lieu où l'on a observé la grande hauteur.

Je dois rappeller ici la dernière réflexion que nous avons faite par rapport à la méthode précédente, car elle a lieu également pour celle-ci. Pour un plus grand éclaircissement, déduisons la relation entre l'erreur du résultat, et une erreur supposée dans l'intervalle, quand on procure l'identité de la latitude estimée, et de la latitude calculée, en employant les formules de la présente solution.

On aura

$$\mp \partial L = \frac{\partial P (\text{sin. } b' - \text{sin. } t - \text{tan. } d \text{ cot. } l \text{ sin. } b)}{\text{sin. } b' (\text{tan. } d \text{ cot. } l \sim 1)} + \frac{\partial t \text{ sin. } b}{\text{tan. } d \sim \text{tan. } l}$$

D'où, parceque $\partial L = \partial l''$, on déduira

$$\mp \partial P \text{ sin. } b' (\text{tan. } d \sim \text{tan. } l) - \partial P \text{ tan. } l (\text{sin. } b' - \text{sin. } t - \text{tan. } d \text{ cot. } l \text{ sin. } b) = \partial t \text{ sin. } b \text{ sin. } b'$$

et $$- \partial P \text{ tan. } d \text{ sin. } b' + \partial P \text{ tan. } l \text{ sin. } t + \partial P \text{ tan. } d \text{ sin. } b = \partial t \text{ sin. } b \text{ sin. } b'$$

$$\partial l'' = \frac{\partial t \text{ sin. } b \text{ sin. } b'}{\text{tan. } l \text{ sin. } t + \text{tan. } d (\text{sin. } b - \text{sin. } b')}$$

$$\partial l'' = \frac{\partial t \text{ sin. } b \text{ sin. } b'}{\text{tan. } l (\text{sin. } b' \text{cos. } b \perp \text{cos. } b' \text{ sin. } b) - \text{tan. } d (\text{sin. } b' - \text{sin. } b)}$$

$$\partial l'' = \frac{\partial t}{\text{tan. } l (\text{cot. } b - \text{cot. } b') - \text{tan. } d (\text{cosec. } b - \text{cosec. } b')}$$

Expressions égales à celles que nous avons trouvé par les formules des deux méthodes précédentes.

Méthodes indirectes par des Équations relatives à l'Intervalle.

Avec les latitudes estimées, et les données du problème, on pourra calculer le grand angle horaire (ou celui qui répond à la petite hauteur), et le petit horaire (ou celui qui répond à la grande hauteur). En comparant l'intervalle mesuré par la montre avec l'intervalle qui résulte de ces horaires, on aura une différence ∂t; et de-là on déterminera l'équation qu'on doit appliquer à la latitude supposée, en employant la formule $\partial l'' = \frac{\partial t}{\text{tan. } P \text{ (cot. } b - \text{cot. } b') - \text{tan. } d \text{ (cosec. } b - \text{cosec. } b')}$, qui se réduit à $\partial l'' = \frac{\partial t \text{ sin. } b \text{ sin. } b'}{\text{tan. } P \text{ sin. } (b' - b) - 2 \text{ tan. } d \text{ sin. } \frac{1}{2}(b' - b) \text{ cos. } \frac{1}{2}(b' + b)}$,* ou toute autre formule qui donne la relation entre ∂t, et $\partial l''$.

Voici une autre méthode qui me paroît préférable. Faites le calcul du petit horaire avec deux latitudes supposées L, L′ qui ne diffèrent pas beaucoup de la latitude estimée l'' du lieu où l'on a observé la grande hauteur; et appellons les horaires b, K. Faites aussi le calcul du grand horaire avec deux latitudes supposées qui s'éloignent de la latitude l''', où l'on a observé la petite hauteur, de la même quantité et dans le même sens que L, L′ de l''; et appellons ces horaires b', K′. En représentant

* Mon savant ami le Dr. Maskelyne a publié depuis très long tems une autre formule qui détermine cette relation en termes des azimuths. En représentant par P l'angle formé par les verticaux de l'astre aux instants des observations, il trouve $\partial P = \frac{\partial t \text{ cos. } l' \text{ sin. } c \text{ sin. } c'}{\text{sin. } P}$. Notre auteur a déduit cette expression par le moyen des analogies différentielles. Je l'ai demontrée d'une autre manière dans mon Mémoire inseré dans la *Connoissance des Tems pour* 1793.

La même formule a été consultée pour établir les régles données, premièrement dans le *British Mariner's Guide*, et copiées depuis dans les *Requisite Tables*, et dans d'autres ouvrages, pour choisir les circonstances qui conviennent à l'usage de la méthode de Douwes.

par t l'intervalle qui résulte de la comparaison de b, et K, par t' l'intervalle qui résulte de la comparaison de b', et K', et par T l'intervalle mesuré par la montre, on aura $\partial L : \partial L' :: \partial t : \partial t'$ et par conséquent $\partial L = \frac{(\partial L \gtrless \partial L')\,\partial t}{\partial t \gtrless \partial t'}$

qui se réduit à $\partial L = \frac{(L \sim L')\,(t \sim T)}{t \sim t'}$

C'est l'équation qu'on doit appliquer à la latitude supposée L. La latitude vraie sera comprise, ou non, entre les deux latitudes supposées, selon que les deux intervalles calculés différeront de l'intervalle observé dans des sens opposés, ou dans le même sens. Et dans le dernier cas, la latitude vraie sera plus près de la latitude supposée dont l'intervalle correspondant différera le moins de celui que donne la montre.

Toutes les solutions par des équations relatives à l'intervalle ont, cependant, un grand inconvénient; car elles supposent qu'on connoîsse à quel coté du méridien appartient la plus grande hauteur. Et, comme ce cas douteux arrive précisément quand on a observé près du midi, c'est-à-dire, dans les circonstances les plus favorables pour l'exactitude du résultat, je ne crois pas qu'on puisse adopter dans la pratique ces sortes de procédés, surtout, quand on possède d'autres méthodes, qui réunissent toutes les propriétés réquises.

La Latitude du Lieu, ainsi que la Hauteur, et la Déclinaison d'un Astre étant données, trouver son Angle horaire.

La Trigonométrie Sphérique donne

$$\cos. b = \frac{\sin. a - \sin. d \sin. l}{\cos. d \cos. l}$$

par conséquent

$$\sin. v. b = 1 - \frac{\sin. a - \sin. d \sin. l}{\cos. d \cos. l}$$

$$\text{sin. v. } b = \frac{\text{cos. } d \text{ cos. } l + \text{sin. } d \text{ sin. } l - \text{sin. } a}{\text{cos. } d \text{ cos. } l}$$

$$\text{sin. v. } b = \frac{\text{cos. } (d \sim l) - \text{sin. } a}{\text{cos. } d \text{ cos. } l}$$

$$\text{sin. v. } b = \frac{\text{cos. } (d \sim l) - \text{cos. } (90° - a)}{\text{cos. } d \text{ cos. } l}$$

$$\text{sin. v. } b = \frac{2 \text{ sin. } \frac{1}{2} \left(90° - a + (d \sim l)\right) \text{sin. } \frac{1}{2} \left(90° - a - (d \sim l)\right)}{\text{cos. } d \text{ cos. } l}$$

Expression propre pour employer les logarithmes sinus-verses, et ceux des doubles-sinus.

On a aussi

$$\text{sin. v. } b = \frac{\sqrt{\text{cos. v. } \left(a - (d \sim l)\right) \text{cos. v. } \left(a + (d \sim l)\right)}}{\text{cos. } d \text{ cos. } l}$$

Pour employer les logarithmes sinus et tangentes, on déduit

$$\text{sin. } \tfrac{1}{2} b = \sqrt{\frac{\text{sin. } \frac{1}{2} \left(90° - a + (d \sim l)\right) \text{sin. } \frac{1}{2} \left(90° - a - (d \sim l)\right)}{\text{cos. } d \text{ cos. } l}}$$

Cette formule, et l'avant-dernière, ont un avantage assez considérable, quand on emploie des tables, comme celles de SHERWIN et de GARDINER, où il faut prendre des parties proportionelles; car elles sont additives pour les sinus et les sécantes, et par conséquent on peut les mettre au dessous des logarithmes correspondants aux arguments les plus proches, et faire ensuite une addition de tous ces nombres.

En employant les tables de TAYLOR, le calcul seroit un peu plus court par la formule suivante.

$$\text{sin. } \tfrac{1}{2} b = \sqrt{\frac{\text{cos. } \frac{1}{2} \left(90° + a + (d \sim l)\right) \text{cos. } \frac{1}{2} \left(90° + a - (d \sim l)\right)}{\text{cos. } d \text{ cos. } l}}$$

Si l'on substitue l'expression de la distance polaire D, au lieu de $90° \sim d$, on aura

$$\text{sin } \tfrac{1}{2} b = \sqrt{\frac{\text{cos. } \frac{1}{2} (D + l + a) \text{ sin. } \frac{1}{2} (D + l - a)}{\text{cos. } l \text{ sin. } D}}$$

Celle-ci est la formule de M. DE BORDA, qu'on trouve dans différens ouvrages.

La Latitude géographique, ainsi que la Déclinaison, et l'Angle horaire d'un Astre étant donnés, trouver sa Hauteur.

Nous avons trouvé

$$\text{cos.}\,(d \sim l) = \text{sin.}\,a + \text{sin. v.}\,b\ \text{cos.}\,d\ \text{cos.}\,l$$

Par conséquent

$$\text{sin.}\,a = \text{cos.}\,(d \sim l) - \text{sin. v.}\,b\ \text{cos.}\,d\ \text{cos.}\,l$$

et $$\text{sin.}\,a = \text{cos.}\,(d \sim l)\left(1 - \frac{\text{sin. v.}\,b\ \text{cos.}\,d\ \text{cos.}\,l}{\text{cos.}\,(d \sim l)}\right)$$

En faisant donc $\frac{\text{sin. v.}\,b\ \text{cos.}\,d\ \text{cos.}\,l}{\text{cos.}\,(d \sim l)} = \text{sin. v.}\,\text{N}$, on aura

$$\text{sin.}\,a = \text{cos.}\,(d \sim l)\ \text{cos.}\,\text{N}.$$

Et l'on voit, que quand sin. v. N est plus grand que le rayon, l'astre est sous l'horizon, et la hauteur calculée est négative.

Nous avons trouvé aussi

$$\text{sin. v.}\,(d \sim l) = \text{cos. v.}\,a - \text{sin. v.}\,b\ \text{cos.}\,d\ \text{cos.}\,l$$

Par conséquent

$$\text{sin.}^2\tfrac{1}{2}\,(d \sim l) = \text{cos.}^2\tfrac{1}{2}\,(90^\circ + a) - \text{sin.}^2\tfrac{1}{2}\,b\ \text{cos.}\,d\ \text{cos.}\,l$$

et $$\text{cos.}\,\tfrac{1}{2}\,(90^\circ + a) = \text{sin.}\,\tfrac{1}{2}\,(d \sim l)\sqrt{1 + \frac{\text{sin.}^2\tfrac{1}{2}\,b\ \text{cos.}\,d\ \text{cos.}\,l}{\text{sin.}^2\tfrac{1}{2}\,(d \sim l)}}$$

En faisant donc $\frac{\text{sin.}\,\frac{1}{2}\,b\sqrt{\text{cos.}\,d\ \text{cos.}\,l}}{\text{sin.}\,\frac{1}{2}\,(d \sim l)} = \text{tan.}\,\text{N}$, on aura

$$\text{cos.}\,\tfrac{1}{2}\,(90^\circ + a) = \frac{\text{sin.}\,\frac{1}{2}\,(d \sim l)}{\text{cos.}\,\text{N}}.$$

De l'équation établie ci-dessus relative à susin. v. $(d \sim l)$ on tire également

$$\text{susin. v.}\,(d \sim l) = \text{sucos. v.}\,a + \text{sin. v.}\,b\ \text{cos.}\,d\ \text{cos.}\,l$$

$$\text{cos.}^2\tfrac{1}{2}\,(d \sim l) = \text{sin.}^2\tfrac{1}{2}\,(90^\circ + a) + \text{sin.}^2\tfrac{1}{2}\,b\ \text{cos.}\,d\ \text{cos.}\,l$$

$$\text{sin.}\,\tfrac{1}{2}\,(90^\circ + a) = \text{cos.}\,\tfrac{1}{2}\,(d \sim l)\sqrt{1 - \frac{\text{sin.}^2\tfrac{1}{2}\,b\ \text{cos.}\,d\ \text{cos.}\,l}{\text{cos.}\,\tfrac{1}{2}\,(d \sim l)}}$$

En faisant donc $\frac{\sin.\frac{1}{2}b\sqrt{\cos.d\cos.l}}{\cos.\frac{1}{2}(d\sim l)} = \cos.\text{N}$, on aura $\sin.\frac{1}{2}(90°+a) = \cos.\frac{1}{2}(d\sim l)\cos.\text{N}$.

Cette formule est plus commode pour la pratique que la précédente relative à $\cos.\frac{1}{2}(90°+a)$. Et toutes les deux sont propres pour faire le calcul par les tables des logarithmes sinus et tangentes.

SECONDE PARTIE.

*La Distance apparente de la Lune au Soleil, ou à une Étoile, et les Hauteurs des deux Astres étant données, trouver leur Distance corrigée des Effets de la Réfraction et de la Parallaxe.**

Les mesures prises par le Bureau des Longitudes de la Grande Bretagne, pour faire calculer et publier un Almanach Nautique, avec les distances de la lune au soleil, et à plusieurs étoiles, forment une époque remarquable dans l'histoire de la Navigation, et l'utilité reconnue de cet établissement fait un grand honneur à la nation qui a fourni par là des moyens de sureté aux Navigateurs de tout le Monde. Quand ces Ephemerides eurent annoncé les élémens nécessaires avec assez d'anticipation et d'exactitude, il devint important de trouver des formules propres pour abréger la réduction des distances lunaires apparentes, afin de les dégager des effets de la réfraction et de la parallaxe. L'Abbé de

* La grande utilité de ce problème a engagé un grand nombre de géometres et d'astronomes à s'en occuper, et l'on doit des solutions à l'Abbé DE LA CAILLE, au Dr. MASKELYNE, au grand EULER, à M. DE BORDA, à M. LEXELL, à M. DE LA GRANGE, à M. FUSS, à M. KRAFFT, à &c. &c. &c. J'ai eu la curiosité de suivre les progrés de ces recherches, mais l'histoire en est trop longue pour l'insérer ici, et je dois la remettre à une autre occasion.

la CAILLE avoit donné une méthode d'approximation; mais elle se borne aux équations qui dépendent des premières dimensions, ce qui n'est pas assez exact pour la pratique. Le Dr. MASKELYNE, à qui l'Astronomie Nautique a tant d'obligations, est le premier qui a perfectionné la solution approchée, en la poussant jusqu'à l'exactitude, et en inventant des formules pour abréger les opérations numériques. On a donné depuis différentes formes aux expressions des corrections qu'on doit appliquer à la distance observée; mais le désir de produire des nouveautés s'est souvent emparé des personnes qui manquoient de tact et de principes, et on les a vu étaler à ce sujet des régles fausses ou inexactes, qui quelquefois ont séduit les Pilotes. Tous ceux qui cultivent l'étude de la Navigation, ont sans doute rencontré des cas pareils, et se sont vu forcés quelques fois d'examiner ces idées empiriques. Quant à moi, le regret du tems que j'ai perdu à considérer chaque solution particulière qui m'a tombé sous les mains, m'a fait penser à la fin à établir des formules générales qui pussent servir de modèle, ou de termes de comparaison, pour déterminer si une méthode quelconque est vraie, ou fausse, ou bien à quel point elle est exacte. Ce projet m'a premièrement engagé dans l'analyse de la solution par approximation; et c'est aussi sous ce point de vue principal que j'ai rédigé cet article de mes Recherches.

On a aussi cherché des méthodes directes pour résoudre ce problème. Mr. DUNTHORNE en publia une de cette espèce dans les *Requisite Tables* de 1767; mais ses opérations exigent l'usage combiné des nombres naturels et artificiels. M. DE BORDA est le premier à qui l'on est redevable d'un procédé direct pour faire ce calcul par le seul moyen des logarithmes. On a depuis proposé quelques autres méthodes; mais ne pou-

vant pas les détailler ici, je me contenterai de faire ci-après mention des principales.

Je dois remarquer, que je me suis borné aux expressions qu'on peut calculer, soit par les logarithmes, soit par les nombres naturels, et que je ne me suis pas occupé de celles dont les opérations exigeroient la combinaison de ces deux moyens de calcul.

Je donnerai d'abord la théorie générale des méthodes directes, et je m'occuperai ensuite de l'analyse des solutions par approximation.

Soient, pour cette Seconde Partie, a la hauteur apparente, et A la hauteur vraie de la lune, b la hauteur apparente, et H la hauteur vraie du soleil, ou de l'étoile, d la distance apparente, et D la distance vraie des deux astres.

Méthodes Directes.

En représentant par Z l'angle au zénith, formé par les verticaux des deux astres, et considérant le triangle formé par la distance, et les complements des hauteurs apparentes, on aura

$$\text{cos. D} = \text{cos. Z cos. A cos. H} + \text{sin. A sin. H}$$

Et considérant le triangle formé par les élémens vrais

$$\text{cos. Z} = \frac{\text{cos. } d - \text{sin. } a \text{ sin. } b}{\text{cos. } a \text{ cos. } b}$$

Par conséquent, en substituant cette expression dans la première équation, on déduira

$$\text{cos. D} = (\text{cos. } d - \text{sin. } a \text{ sin. } b)\,\frac{\text{cos. A cos. H}}{\text{cos. } a \text{ cos. } b} + \text{sin. A sin. H.}$$

Voilà l'expression générale de la relation entre la distance vraie et les données du problème. Il s'agit de chercher des formules propres pour l'usage des logarithmes, ou des nombres naturels.

La quantité sin. a sin. b est $= -\cos.(a+b)+\cos. a \cos. b$, ou bien $=\cos.(a\sim b)-\cos. a \cos. b$. On pourra, donc, substituer l'une ou l'autre de ces expressions; et de-là résultent deux suites de transformations de l'équation fondamentale, la première par les sommes, la seconde par les différences. De chaque équation de cos. D on peut aussi conclure une valeur de sin. v. D, et une valeur correspondante de susin. v. D; car sin. v. $D = 1-\cos. D$, et susin. v. $D = 1+\cos. D$; et de cette manière les solutions se ramifient encore en deux autres branches. Nous suivrons cette marche pour parvenir aux formules que nous cherchons.

En substituant la première expression sin. a sin. $b = -\cos.(a+b)+\cos. a \cos. b$, on aura, par les sommes,

$$\cos. D = \left\{\begin{array}{l}\cos. d+\cos.(a+b)\\ -\cos. a \cos. b\end{array}\right\}\Big/\frac{\cos. A \cos. H}{\cos. a \cos. b}+\sin. A \sin. H.$$

$$\cos. D = \left\{\begin{array}{l}\cos. d+\cos.(a+b)\\ \cos. A \cos. H\end{array}\right\}\Big/\frac{\cos. A \cos. H}{\cos. a \cos. b}-\cos. A \cos. H+\sin. A \sin. H.$$

$$\cos. D = \left\{\begin{array}{l}\cos. d+\cos.(a+b)\\ \frac{\cos. A \cos. H}{\cos. a \cos. b}\end{array}\right\}-\cos.(A+H)$$

$$\cos. D = 2\cos.\tfrac{1}{2}(d+a+b)\cos.\tfrac{1}{2}\left\{d\sim(a+b)\right\}\Big/\frac{\cos. A \cos. H}{\cos. a \cos. b}-\cos.(A+H)$$

Par conséquent

1re Formule.

$$\cos. D = 2\cos.\tfrac{1}{2}(d+a+b)\cos.\tfrac{1}{2}\left\{d\sim(a+b)\right\}\Big/\frac{\cos. A \cos. H}{\cos. a \cos. b}\left(1-\frac{\cos.(A+H)}{2\cos.\tfrac{1}{2}(d+a+b)\cos.\tfrac{1}{2}\left\{d\sim(a+b)\right\}\Big/\frac{\cos. A \cos. H}{\cos. a \cos. b}}\right)$$

En faisant, donc,

$$\frac{\cos.(A+H)}{2\cos.\tfrac{1}{2}(d+a+b)\cos.\tfrac{1}{2}\left\{d\sim(a+b)\right\}\Big/\frac{\cos. A \cos. H}{\cos. a \cos. b}} = \text{sin. v. N.}$$

on aura

$$\cos. D = 2\cos.\tfrac{1}{2}(d+a+b)\cos.\tfrac{1}{2}\left\{d\sim(a+b)\right\}\Big/\frac{\cos. A. \cos. H}{\cos. a \cos. b}\cos. N.$$

La présente méthode exige quelques modifications, car

cos. (A + H) change de signe quand A + H excède 90°. Mais je ne m'arrêterai pas à détailler les distinctions des cas qu'on devroit faire dans cette formule, et dans quelques unes de celles qui y dérivent, car cette seule circonstance suffit pour les abandonner dans la pratique.

De l'expression précédente de cos. D on tire

$$\text{sin.v.D} = \text{susin.v.}(\text{A}+\text{H}) - 2\cos.\tfrac{1}{2}(d+a+b)\cos.\tfrac{1}{2}\left(d\sim(a+b)\right)\frac{\cos.\text{A}\cos.\text{H}}{\cos.a\cos.b}$$

Cette équation fournit les trois formules suivantes.

2me Formule.

En faisant

$$2\cos.\tfrac{1}{2}(d+a+b)\cos.\tfrac{1}{2}\left(d\sim(a+b)\right)\frac{\cos.\text{A}\cos.\text{H}}{\cos.a\cos.b} = \text{susin.v. N}$$

ou bien,

$$\sqrt{\cos.\tfrac{1}{2}(d+a+b)\cos.\tfrac{1}{2}\left(d\sim(a+b)\right)\frac{\cos.\text{A}\cos.\text{H}}{\cos.a\cos.b}} = \cos.\text{N}'$$

on aura $\text{sin.v. D} = \sqrt{\text{sin.v.}(\text{N}+\text{A}+\text{H})\,\text{sin.v.}\left(\text{N}-(\text{A}+\text{H})\right)}$

ou $\text{sin.v. D} = 2\,\text{sin.}\left(\text{N}'+\tfrac{1}{2}(\text{A}+\text{H})\right)\text{sin.}\left(\text{N}'-\tfrac{1}{2}(\text{A}+\text{H})\right)$

3me Formule.

En faisant

$$2\cos.\tfrac{1}{2}(d+a+b)\cos.\tfrac{1}{2}\left(d\sim(a+b)\right)\frac{\cos.\text{A}\cos.\text{H}}{\cos.a\cos.b} = \text{sin.v. N}$$

ou bien,

$$\sqrt{\cos.\tfrac{1}{2}(d+a+b)\cos.\tfrac{1}{2}\left(d\sim(a+b)\right)\frac{\cos.\text{A}\cos.\text{H}}{\cos.a\cos.b}} = \text{sin. N}'$$

on aura $\text{sin.v. D} = \sqrt{\text{susin.v.}(\text{N}+\text{A}+\text{H})\,\text{susin.v.}\left(\text{N}\sim(\text{A}+\text{H})\right)}$

ou $\text{sin.v. D} = 2\cos.\left(\text{N}'+\tfrac{1}{2}(\text{A}+\text{H})\right)\cos.\left(\text{N}'\sim\tfrac{1}{2}(\text{A}+\text{H})\right)$

4me Formule.

L'équation précédente se réduit à

$$\text{sin.v. D} = \text{susin.v. (A+H)}\left(1 - \frac{2\cos.\frac{1}{2}(d+a+b)\cos.\frac{1}{2}\left(d\sim(a+b)\right)\cos.\text{A}\cos.\text{H}}{\text{susin.v. (A+H)}\cos.a\cos.b}\right)$$

En faisant, donc, $\frac{2\cos.\frac{1}{2}(d+a+b)\cos.\frac{1}{2}\left(d\sim(a+b)\right)\cos.\text{A}\cos.\text{H}}{\text{susin.v. (A+H)}\cos.a\cos.b} = \cos.\text{N}$

on aura $\text{sin. v. D} = \text{susin. v. (A + H) sin. v. N.}$

5me Formule.

De la même expression de cos. D on déduit aussi

$$\text{susin.v.D} = \text{sin.v.(A+H)} + 2\cos.\tfrac{1}{2}(d+a+b)\cos.\tfrac{1}{2}\left(d\sim(a+b)\right)\frac{\cos.\text{A}\cos.\text{H}}{\cos.a\cos.b}$$

$$\text{susin.v.D} = \text{sin.v.(A+H)}\left(1 + \frac{2\cos.\frac{1}{2}(d+a+b)\cos.\frac{1}{2}\left(d\sim(a+b)\right)\cos.\text{A}\cos.\text{H}}{\text{sin. v. (A+H)}\cos.a\cos.b}\right)$$

En faisant, donc, $\frac{2\cos.\frac{1}{2}(d+a+b)\cos.\frac{1}{2}\left(d\sim(a+b)\right)\cos.\text{A}\cos.\text{H}}{\text{sin. v. (A+H)}\cos.a\cos.b} = \cos.\text{N}$

on aura $\text{susin. v. D} = \text{sin. v. (A + H) susin. v. N.}$

Je remarquerai ici, qu'on pourroit substituer dans les formules de ces méthodes $\sqrt{\text{susin. v. } (d+a+b)\text{ susin. v. }\left(d\sim(a+b)\right)}$, à la place de $2\cos.\frac{1}{2}(d+a+b)\cos.\frac{1}{2}\left(d\sim(a+b)\right)$, pour employer les susinus-verses au lieu des cosinus.

En substituant la seconde expression $\cos.(a\sim b) - \cos.a\cos.b = \text{sin. } a \text{ sin. } b$, dans la formule fondamentale, on aura par les différences

$$\cos.\text{D} = \left(\cos.d - \cos.(a\sim b) + \cos.a\cos.b\right)\frac{\cos.\text{A}\cos.\text{H}}{\cos.a\cos.b} + \text{sin. A sin. H}$$

$$\cos.\text{D} = \left(\cos.d - \cos.(a\sim b)\right)\frac{\cos.\text{A}\cos.\text{H}}{\cos.a\cos.b} + \cos.(\text{A}\sim\text{H})$$

$$\cos.\text{D} = \cos.(\text{A}\sim\text{H}) - 2\sin.\tfrac{1}{2}\left(d+(a\sim b)\right)\sin.\tfrac{1}{2}\left(d-(a\sim b)\right)\frac{\cos.\text{A}\cos.\text{H}}{\cos.a\cos.b}$$

Par conséquent:

6me Formule.

$$\cos. D = \cos. (A \sim H) \left(1 - \frac{2 \sin. \frac{1}{2}\left(d + (a \sim b)\right) \sin. \frac{1}{2}\left(d - (a \sim b)\right) \cos. A \cos. H}{\cos. (A \sim H) \cos. a \cos. b}\right)$$

En faisant, donc, $\frac{2 \sin. \frac{1}{2}\left(d + (a \sim b)\right) \sin. \frac{1}{2}\left(d - (a \sim b)\right) \cos. A \cos. H}{\cos. (A \sim H) \cos. a \cos. b} = \text{sin.v. N}$

on aura $\cos. D = \cos. (A \sim H) \cos. N.$

Et D sera plus grand ou moindre que 90°, selon que sin.v. N sera plus grand ou plus petit que le rayon.

De l'expression précédente de cos. D on tire ce qui suit.

7me Formule.

$$\text{sin.v.D} = \text{sin.v.}(A \sim H) + 2 \sin. \tfrac{1}{2}\left(d + (a \sim b)\right) \sin. \tfrac{1}{2}\left(d - (a \sim b)\right) \frac{\cos. A \cos. H}{\cos. a \cos. b}$$

$$\text{sin.v.D} = \text{sin.v.}(A \sim H) \left(1 + \frac{2 \sin. \frac{1}{2}\left(d + (a \sim b)\right) \sin. \frac{1}{2}\left(d - (a \sim b)\right) \cos. A \cos. H}{\text{sin. v. } (A \sim H) \cos. a \cos. b}\right)$$

En faisant, donc, $\frac{2 \sin. \frac{1}{2}\left(d + (a \sim b)\right) \sin. \frac{1}{2}\left(d - (a \sim b)\right) \cos. A \cos. H}{\text{sin. v. } (A \sim H) \cos. a \cos. b} = \cos. N$

on aura $\text{sin. v. D} = \text{sin. v. } (A \sim H) \text{ susin. v. N.}$

De la même expression de cos. D on déduit aussi

$$\text{susin.v.D} = \text{susin.v.}(A \sim H) - 2 \sin. \tfrac{1}{2}\left(d + (a \sim b)\right) \sin. \tfrac{1}{2}\left(d - (a \sim b)\right) \frac{\cos. A \cos. H}{\cos. a \cos. b}$$

Cette équation fournit les trois formules suivantes.

8me Formule.

En faisant

$$2 \sin. \tfrac{1}{2}\left(d + (a \sim b)\right) \sin. \tfrac{1}{2}\left(d - (a \sim b)\right) \frac{\cos. A \cos. H}{\cos. a \cos. b} = \text{susin. v. N}$$

ou bien,

$$\sqrt{\sin. \tfrac{1}{2}\left(d + (a \sim b)\right) \sin. \tfrac{1}{2}\left(d - (a \sim b)\right) \frac{\cos. A \cos. H}{\cos. a \cos. b}} = \cos. N'$$

on aura $\text{susin.v. D} = \sqrt{\text{sin.v.}\left(N + (A \sim H)\right) \text{sin.v.}\left(N - (A \sim H)\right)}$

ou $\text{susin. v. D} = 2 \text{ sin.} \left(N' + \tfrac{1}{2}(A \sim H)\right) \text{sin.} \left(N' - \tfrac{1}{2}(A \sim H)\right).$

9me Formule.

En faisant

$$2\sin.\tfrac{1}{2}\left(d+(a\sim b)\right)\sin.\tfrac{1}{2}\left(d-(a\sim b)\right)\frac{\text{cos. A. cos. H}}{\text{cos. }a\text{ cos. }b}=\text{sin. v. N}$$

ou bien,

$$\sqrt{\sin.\tfrac{1}{2}\left(d+(a\sim b)\right)\sin.\tfrac{1}{2}\left(d-(a\sim b)\right)}\,\frac{\text{cos. A cos. H}}{\text{cos. }a\text{ cos. }b}=\text{sin. N}'$$

on aura

$$\text{susin v. D}=\sqrt{\text{susin. v.}\left(\text{N}+(\text{A}\sim\text{H})\right)\text{susin. v.}\left(\text{N}\sim(\text{A}\sim\text{H})\right)}$$

ou susin. v. D $=2\cos.\left(\text{N}'+\tfrac{1}{2}(\text{A}\sim\text{H})\right)\cos.\left(\text{N}'\sim\tfrac{1}{2}(\text{A}\sim\text{H})\right)$

10me Formule.

L'équation précédente se réduit à

$$\text{susin.v.D}=\text{susin.v.}(\text{A}\sim\text{H})\left(1-\frac{2\sin.\frac{1}{2}\left(d+(a\sim b)\right)\sin.\frac{1}{2}\left(d-(a\sim b)\right)\text{cos. A cos. H}}{\text{susin. v. }(\text{A}\sim\text{H})\text{ cos. }a\text{ cos. }b}\right)$$

En faisant, donc, $\frac{2\sin.\frac{1}{2}\left(d+(a\sim b)\right)\sin.\frac{1}{2}\left(d-(a\sim b)\right)\text{cos. A cos. H}}{\text{susin. v. }(\text{A}\sim\text{H})\text{ cos. }a\text{ cos. }b}=\text{cos. N}$

on aura susin. v. D $=$ susin. v. (A$\sim$H) sin. v. N.

Je remarquerai, qu'on pourroit aussi substituer dans les quatre formules précédentes $\sqrt{\text{sin. v.}\left(d+(a\sim b)\right)\text{sin. v.}\left(d-(a\sim b)\right)}$ au lieu de $2\sin.\frac{1}{2}\left(d+(a\sim b)\right)\sin.\frac{1}{2}\left(d-(a\sim b)\right)$.

Les formules que nous venons d'établir sont propres pour le calcul par les logarithmes sinus-verses, et l'on pourroit combiner aussi l'usage des logarithmes doubles-sinus. Cherchons à présent des expressions pour employer seulement les logarithmes sinus et tangentes, tels qu'on les trouve dans les Tables de GARDINER et de TAYLOR.

11me Formule.

Dans la 1re formule on pourroit faire

$$\sqrt{\frac{\cos.(A+H)}{4\cos.\frac{1}{2}(d+a+b)\cos.\frac{1}{2}\left(d\sim(a+b)\right)\frac{\cos.A\cos.H}{\cos.a\cos.b}}}=\sin.N$$

pour avoir

$$\cos.D=2\cos.\tfrac{1}{2}(d+a+b)\cos.\tfrac{1}{2}\left(d\sim(a+b)\right)\frac{\cos.A\cos.H}{\cos.a\cos.b}\cos.^2N$$

Mais je dois rappeller ici ce que j'ai dit à l'occasion de la 1re méthode.

12me Formule.

En substituant dans la 2me formule $2\sin.^2\frac{1}{2}D=\sin.v.D$, et faisant aussi

$$\sqrt{\cos.\tfrac{1}{2}(d+a+b)\cos.\tfrac{1}{2}\left(d\sim(a+b)\right)\frac{\cos.A\cos.H}{\cos.a\cos.b}}=\cos.N'$$

on aura

$$\sin.\tfrac{1}{2}D=\sqrt{\sin.\left(N'+\tfrac{1}{2}(A+H)\right)\sin.\tfrac{1}{2}\left(N'\sim\tfrac{1}{2}(A+H)\right)}$$

13me Formule.

En faisant, comme dans la 3me méthode,

$$\sqrt{\cos.\tfrac{1}{2}(d+a+b)\cos.\tfrac{1}{2}\left(d\sim(a+b)\right)\frac{\cos.A\cos.H}{\cos.a\cos.b}}=\sin.N'$$

on aura

$$\sin.\tfrac{1}{2}D=\sqrt{\cos.\left(N'+\tfrac{1}{2}(A+H)\right)\cos.\left(N'\sim\tfrac{1}{2}(A+H)\right)}$$

14me Formule.

En substituant dans la 4me formule

$2\sin.^2\frac{1}{2}D=\sin.v.D$, et $2\cos.^2\frac{1}{2}(A+H)=\text{susin.v.}(A+H)$,

on tire

$$\sin.^2\tfrac{1}{2}D=\cos.^2\tfrac{1}{2}(A+H)\left(1-\frac{\cos.\frac{1}{2}(d+a+b)\cos.\frac{1}{2}\left(d\sim(a+b)\right)\cos.A\cos.H}{\cos.^2\frac{1}{2}(A+H)\cos.a\cos.H}\right)$$

$$\sin.\tfrac{1}{2}D=\cos.\tfrac{1}{2}(A+H)\sqrt{1-\frac{\cos.\frac{1}{2}(d+a+b)\cos.\frac{1}{2}\left(d\sim(a+b)\right)\cos.A\cos.H}{\cos.^2\frac{1}{2}(A+H)\cos.a\cos.b}}$$

En faisant, donc,

$$\frac{1}{\cos.\frac{1}{2}(A+H)}\sqrt{\frac{\cos.\frac{1}{2}(d+a+b)\cos.\frac{1}{2}\left(d\sim(a+b)\right)\cos.A\cos.H}{\cos.a\cos.b}}=\sin.N$$

on aura $\sin.\frac{1}{2}D=\cos.\frac{1}{2}(A+H)\cos.N.$

Cette méthode est celle de M. DE BORDA, que les Navigateurs du continent employent avec succès depuis plusieurs années.

15me Formule.

De la 5me formule on déduit

$$\cos.\tfrac{1}{2}D=\sin.\tfrac{1}{2}(A+H)\sqrt{1+\frac{\cos.\frac{1}{2}(d+a+b)\cos.\frac{1}{2}\left(d\sim(a+b)\right)\cos.A\cos.H}{\sin.^2\frac{1}{2}(A+H)\cos.a\cos.b}}$$

En faisant, donc,

$$\frac{1}{\sin.\frac{1}{2}(A+H)}\sqrt{\frac{\cos.\frac{1}{2}(d+a+b)\cos.\frac{1}{2}\left(d\sim(a+b)\right)\cos.A\cos.H}{\cos.a\cos.b}}=\tan.N$$

on aura $\cos.\frac{1}{2}D=\frac{\sin.\frac{1}{2}(A+H)}{\cos.N}.$

16me Formule.

Par les formules de la 6me méthode on voit, qu'en faisant

$$\sqrt{\frac{\sin.\frac{1}{2}\left(d+(a\sim b)\right)\sin.\frac{1}{2}\left(d-(a\sim b)\right)\cos.A\cos.H}{\cos.(A\sim H)\cos.a\cos.b}}=\sin.N$$

on aura $\cos.D=\cos.(A\sim H)\cos.2N.$

Et la distance D sera toujours de la même espèce que l'arc 2N.

17*me Formule.*

De la 7me formule on déduit

$$\sin.\tfrac{1}{2}D=\sin.\tfrac{1}{2}(A\sim H)\sqrt{1+\frac{\sin.\frac{1}{2}\left(d+(a\sim b)\right)\sin.\frac{1}{2}\left(d-(a\sim b)\right)\cos.A\cos.H}{\sin.^{2}\frac{1}{2}(A\sim H)\cos.a\cos.b}}$$

En faisant, donc,

$$\frac{1}{\sin.\frac{1}{2}(A\sim H)}\sqrt{\frac{\sin.\frac{1}{2}\left(d+(a\sim b)\right)\sin.\frac{1}{2}\left(d-(a\sim b)\right)\cos.A\cos.H}{\cos.a\cos.b}}=\tan.N$$

on aura $$\sin.\tfrac{1}{2}D=\frac{\sin.\frac{1}{2}(A\sim H)}{\cos.N}.$$

Le Dr. MASKELYNE nous a donné les régles pratiques de cette méthode dans son Introduction aux Tables des Logarithmes de TAYLOR.

18*me Formule.*

En faisant, comme dans la 8me méthode,

$$\sqrt{\sin.\tfrac{1}{2}\left(d+(a\sim b)\right)\sin\tfrac{1}{2}\left(d-(a\sim b)\right)\frac{\cos.A\cos.H}{\cos.a\cos.b}}=\cos.N'$$

on aura $$\cos.\tfrac{1}{2}D=\sqrt{\sin.\left(N'+\tfrac{1}{2}(A\sim H)\right)\sin.\left(N'-\tfrac{1}{2}(A\sim H)\right)}$$

19*me Formule.*

En faisant, comme dans la 9me méthode,

$$\sqrt{\sin.\tfrac{1}{2}\left(d+(a\sim b)\right)\sin.\tfrac{1}{2}\left(d-(a\sim b)\right)\frac{\cos.A\cos.H}{\cos.a\cos.b}}=\sin.N'$$

on aura $$\cos.\tfrac{1}{2}D=\sqrt{\cos.\left(N'+\tfrac{1}{2}(A\sim H)\right)\cos.\left(N'-\tfrac{1}{2}(A\sim H)\right)}.$$

Cette méthode est celle de Mr. DUNTHORNE perfectionnée par le Dr. MASKELYNE, dont les régles de calcul se trouvent dans les *Requisite Tables* de 1781.

20me Formule.

De la 10me formule on déduit

$$\cos.\tfrac{1}{2}D = \cos.\tfrac{1}{2}(A\sim H)\sqrt{1-\frac{\sin.\frac{1}{2}\left(d+(a\sim b)\right)\sin.\frac{1}{2}\left(d-(a\sim b)\right)\cos.A\cos.H}{\cos.^2\frac{1}{2}(A\sim H)\cos.a\cos.b}}$$

En faisant, donc,

$$\frac{1}{\cos.\frac{1}{2}(A\sim H)}\sqrt{\frac{\sin.\frac{1}{2}\left(d+(a\sim b)\right)\sin.\frac{1}{2}\left(d-(a\sim b)\right)\cos.A\cos.H}{\cos.a\cos.b}} = \sin.N$$

on aura $\cos.\frac{1}{2}D = \cos.\frac{1}{2}(A\sim H)\cos.N.$

On remarquera que dans toutes ces formules se trouve la quantité $\frac{\cos.A\cos.H}{\cos.a\cos.b}$, pour laquelle on pourra prendre les différences logarithmiques de DUNTHORNE (Voyez les *Requisite Tables*), au lieu de prendre les quatre logarithmes séparément.

Les méthodes établies pour trouver la demi-distance vraie méritent que nous y fassions quelques reflexions. Les 14me, 15me, 17me, et 20me formules sont les plus commodes; mais on peut demander laquelle d'entre elles est la préférable, ou bien quels sont les avantages ou désavantages de chacune. J'en dirai ici quelques mots, d'autant plus volontiers, que je profiterai de cette occasion pour rectifier quelques opinions prématurées que j'avois eu à ce sujet, faute de l'avoir bien examiné.

La préparation des arguments dans les formules par les sommes est un peu plus courte que dans les formules par les différences; mais cet avantage, à la verité, est très peu considérable, et ne vaut la peine d'y avoir égard, qu'en parité de toutes les autres circonstances.

Il y a deux formules (14me et 20me) où l'on cherche le cosinus de l'angle subsidiaire par le sinus, et deux autres

formules (15me et 17me) où l'on cherche ce cosinus par la tangente; il s'agit de déterminer lequel de ces deux moyens est le plus utile, pour l'exactitude du calcul. Dans les formules par les sinus, on peut supposer une erreur dans cos. $\frac{1}{2}$ (A+H), ou cos. $\frac{1}{2}$ (A∼H); mais la quantité cos. $\frac{1}{2}$ (A+H) cos. N, ou cos. $\frac{1}{2}$ (A∼H) cos. N, étant toujours plus petite, l'erreur résultante sera aussi plus petite. La même chose a lieu relativement à l'erreur qu'on peut commettre, en cherchant cos. N par le moyen de sin. N. Au contraire, dans les formules par la tangente, l'erreur de sin. $\frac{1}{2}$ (A+H), ou sin. $\frac{1}{2}$ (A∼H), produit toujours une erreur plus considérable; car $\frac{\text{sin. } \frac{1}{2}(A+H)}{\text{cos. N}}$, ou $\frac{\text{sin. } \frac{1}{2}(A\sim H)}{\text{cos. N}}$ est plus grand que sin. $\frac{1}{2}$ (A+H), ou sin. $\frac{1}{2}$ (A∼H); et quant à l'erreur de N, l'effet qui y résulte sera plus grand ou plus petit, selon que $\frac{\text{sin. } \frac{1}{2}(A+H)}{\text{cos. N}}$, ou $\frac{\text{sin. } \frac{1}{2}(A\sim H)}{\text{cos. N}}$ sera aussi plus grand ou plus petit que cos. N. On voit, donc, que les formules où il n'y a que des sinus sont préférables à celles qui contiennent la tangente de l'angle subsidiaire.

Nous voilà réduits aux formules 14me et 20me, dont l'une donne le sinus, et l'autre le cosinus de la demi-distance. Pour bien faire, il conviendroit d'employer la première quand la distance est moindre de 90°, et la seconde quand la distance excède le quart de cercle. Mais, en cas qu'on veuille adopter l'une d'entre elles, pour en user généralement sans distinction, on apperçoit que la 14me formule est la plus avantageuse; car les distances que donnent les Ephémerides, étant toujours à peu près entre les limites de 20° et 120°, il vaut mieux chercher les sinus compris entre 10° et 60°, que les cosinus correspondans au même espace, ou les sinus d'entre 30° et 80°. La méthode de M. DE BORDA réunit donc le plus de propriétés utiles, et mérite qu'on la

préfère aux autres dans la pratique, quand on se bornera à une seule manière de calcul, comme les Navigateurs ont coutume de faire.

Nous procéderons à présent à établir des formules pour faire le calcul par les sinus naturels.

Faisons la quantité commune $\frac{\cos. A \cos. H}{\cos. a \cos. b} = 2 \cos. M$, et substituons cette expression dans les formules précédentes. En nous rappellant que $2 \cos. \frac{1}{2}(d+a+b) \cos. \frac{1}{2}\left(d \sim (a+b)\right)$ est $= \cos. d + \cos. (a+b)$, nous aurons, par les sommes, les formules que voici.

21me Formule.

L'expression de cos. D se réduit à

$$\cos. D = \begin{cases} \cos. (d+M) + \cos. (d \sim M) + \cos. (a+b+M) \\ + \cos. \left((a+b) \sim M\right) - \cos. (A+H). \end{cases}$$

22me Formule.

De l'équation fondamentale des 2me, 3me, et 4me formules on déduit

$$\text{sin. v. } D = \begin{cases} \text{susin. v. } (A+H) - \cos. (d+M) - \cos. (d \sim M) \\ - \cos. (a+b+M) - \cos. \left((a+b) \sim M\right). \end{cases}$$

23me Formule.

La formule précédente se réduit à celle-ci

$$\text{sin. v. } D = \begin{cases} \text{susin. v. } (A+H) + \text{sin. v. } (d+M) + \text{sin. v. } (d \sim M) \\ + \text{sin. v. } (a+b+M) + \text{sin. v. } \left((a+b) \sim M\right) - 4. \end{cases}$$

J'ai publié il y a quelque tems cette formule, avec quelques autres notions sur la réduction des distances lunaires.

24me Formule.

La même formule donne

$$\text{sin.v. D} = \begin{cases} \text{susin. v. (A + H)} - \text{susin. v. } (d + \text{M}) \\ - \text{susin. v. } (d \sim \text{M}) - \text{susin. v. } (a + b + \text{M}) \\ - \text{susin. v. } \big((a + b) \sim \text{M}\big) + 4. \end{cases}$$

25me Formule.

La même formule donne aussi

$$\text{sin.v. D} = \begin{cases} -\text{sin.v. (A+H)} + \text{sin.v. } (d+\text{M}) + \text{sin.v. } (d \sim \text{M}) \\ +\text{sin.v. } (a+b+\text{M}) + \text{sin.v. } \big((a+b) \sim \text{M}\big) - 2. \end{cases}$$

26me Formule.

De la 5me formule, on déduit

$$\text{susin.v. D} = \begin{cases} \text{sin. v. (A+H)} + \cos. (d+\text{M}) + \cos. (d \sim \text{M}) \\ +\cos. (a+b+\text{M}) + \cos. \big((a+b) \sim \text{M}\big). \end{cases}$$

27me Formule.

La formule précédente se réduit à celle-ci

$$\text{susin. v. D} = \begin{cases} \text{sin.v. (A+H)} - \text{sin.v. } (d+\text{M}) - \text{sin.v. } (d \sim \text{M}) \\ -\text{sin.v. } (a+b+\text{M}) - \text{sin.v. } \big((a+b) \sim \text{M}\big) + 4. \end{cases}$$

28me Formule.

La même formule donne aussi

$$\text{susin.v. D} = \begin{cases} \text{sin.v. (A+H)} + \text{susin.v. } (d+\text{M}) + \text{susin.v. } (d \sim \text{M}) \\ +\text{susin.v. } (a+b+\text{M}) + \text{susin.v. } \big((a+b) \sim \text{M}\big) \\ -4. \end{cases}$$

29me Formule.

La même formule donne encore

$$\text{susin. v. D} = \begin{cases} -\text{susin. v. (A + H)} + \text{susin. v. } (d + \text{M}) \\ +\text{susin. v. } (d \sim \text{M}) + \text{susin. v. } (a + b + \text{M}) \\ +\text{susin. v. } \big((a + b) \sim \text{M}\big) - 2. \end{cases}$$

En faisant la même substitution de 2 cos. M, et en nous rappellant que $2 \sin. \frac{1}{2}\big(d + (a \sim b)\big) \sin. \frac{1}{2}\big(d - (a \sim b)\big)$ est $= \cos. (a \sim b) - \cos. d$, nous aurons, par les différences, les formules qui suivent.

30me Formule.

L'expression de cos. D se convertit en

$$\cos. \text{D} = \cos. (\text{A} \sim \text{H}) + 2 \cos. \text{M} \cos. d - 2 \cos. \text{M} \cos. (a \sim b)$$

et par conséquent

$$\cos. \text{D} = \begin{cases} \cos. (\text{A} \sim \text{H}) + \cos. (d + \text{M}) + \cos. (d \sim \text{M}) \\ -\cos. \big((a \sim b) + \text{M}\big) - \cos. \big((a \sim b) \sim \text{M}\big) \end{cases}$$

31me Formule.

De la 7me formule on déduit

$$\text{sin. v. D} = \begin{cases} \text{sin. v. (A} \sim \text{H}) - \cos. (d + \text{M}) - \cos. (d \sim \text{M}) \\ +\cos. \big((a \sim b) + \text{M}\big) + \cos. \big((a \sim b) \sim \text{M}\big). \end{cases}$$

Cette formule donne les trois suivantes.

32me Formule.

$$\text{sin. v. D} = \begin{cases} \text{sin. v. (A} \sim \text{H}) + \text{sin. v. } (d + \text{M}) + \text{sin. v. } (d \sim \text{M}) \\ -\text{sin. v. } \big((a \sim b) + \text{M}\big) - \text{sin. v. } \big((a \sim b) \sim \text{M}\big). \end{cases}$$

Mr. KRAFFT nous a donné cette formule dans un beau Mémoire qui fait partie des Actes de l'Académie de Petersbourg.

33me Formule.

$$\text{sin. v. D} = \begin{cases} \text{sin. v. (A} \sim \text{H)} + \text{sin. v. (}d \sim \text{M)} + \text{sin. v. (}d \sim \text{M)} \\ + \text{susin. v.} \big((a \sim b) + \text{M} \big) + \text{susin. v.} \big((a \sim b) \sim \text{M} \big) - 4. \end{cases}$$

34me Formule.

$$\text{sin. v. D} = \begin{cases} - \text{ susin. v. (A} \sim \text{H)} - \text{ susin. v. (}d + \text{M)} \\ - \text{ susin. v. (}d \sim \text{M)} + \text{ susin. v.} \big((a \sim b) + \text{M} \big) \\ + \text{ susin. v.} \big((a \sim b) \sim \text{M} \big) + 2. \end{cases}$$

35me Formule.

De l'équation fondamentale des 8me, 9me, et 10me formules on déduit

$$\text{susin. v. D} = \begin{cases} \text{susin. v. (A} \sim \text{H)} + \text{cos. (}d + \text{M)} + \text{cos. (}d \sim \text{M)} \\ - \text{cos.} \big((a \sim b) + \text{M} \big) - \text{cos.} \big((a \sim b) \sim \text{M} \big). \end{cases}$$

Cette formule donne les trois suivantes.

36me Formule.

$$\text{susin. v. D} = \begin{cases} \text{susin. v. (A} \sim \text{H)} - \text{sin. v. (}d + \text{M)} - \text{sin. v. (}d \sim \text{M)} \\ + \text{sin. v.} \big((a \sim b) + \text{M} \big) + \text{sin. v.} \big((a \sim b) \sim \text{M} \big). \end{cases}$$

37me Formule.

$$\text{susin. v. D} = \begin{cases} \text{susin. v. (A} \sim \text{H)} + \text{ susin. v. (}d + \text{M)} \\ + \text{susin. v. (}d \sim \text{M)} + \text{sin. v.} \big((a \sim b) + \text{M} \big). \\ + \text{sin. v.} \big((a \sim b) \sim \text{M} \big) - 4 \end{cases}$$

38me Formule.

$$\text{susin. v. D} = \begin{cases} -\text{ sin. v. } (\text{A} \sim \text{H}) + \text{susin. v. } (d + \text{M}) \\ +\text{ susin. v. } (d \sim \text{M}) + \text{sin. v. } \big|(a \sim b) + \text{M}\big| \\ +\text{ sin. v. } \big|(a \sim b) \sim \text{M}\big| - 2. \end{cases}$$

Les méthodes, dont les formules renferment des cosinus, ont l'inconvénient de se diviser en différens cas, selon que les arcs correspondans sont plus ou moins grands que le quart de cercle. Toutes les autres formules admettent des régles constantes, et les 23me, 28me, 33me, et 37me réunissent aussi l'avantage de n'exiger que la simple somme des six sinus-verses ou susinus-verses, pour avoir celui de la distance vraie.

Entre les formules par les sommes, et les formules par les différences, les premières sont préférables; car on peut déduire la somme des hauteurs vraies de la somme des hauteurs apparentes d'une manière très simple; pendant que, pour avoir la différence des hauteurs apparentes, il n'y a pas de meilleur procédé que celui de corriger séparément chaque hauteur apparente, pour faire la soustraction ensuite.

J'ai calculé les sinus-verses naturels pour chaque dix secondes de la demi-circonférence, ainsi qu'une table très complette des angles M. Par ces moyens la réduction des distances lunaires deviendra très commode, en employant celle qu'on jugera convenable des formules précédentes.

Pour rendre les opérations encore plus faciles, j'ai calculé une table à double argument (savoir l'angle M, et un autre angle quelconque) qui donne à la fois la quantité

$$\text{sin. v. } (d + \text{M}) + \text{sin. v. } (d \sim \text{M})$$

ou la quantité

$$\text{sin. v. } (d + a + b) + \text{sin. v. } \big|(a + b) \sim \text{M}\big|$$

Ainsi, pour le calcul de la 2gme formule, on réduira les opérations des sinus-verses à la simple somme de trois nombres, et par là on diminuera aussi les opérations préliminaires avec les élémens; car alors il suffira de prendre l'angle auxiliaire (pour argument des nombres sommaires), et de déduire la somme des hauteurs apparentes et des hauteurs vraies.

Voici deux formules par les différences, dont le calcul admet l'application de ces nombres sommaires, quoique d'une manière moins commode que celle de la 2gme méthode.

3gme Formule.

$$\text{sin. v. D} = \begin{cases} \text{sin.v.}(\text{A} \sim \text{H}) + \text{sin.v.}(d + \text{M}) + \text{sin. v.}(d \sim \text{M}) \\ \quad + \text{sin. v.}\Big(180^\circ \sim \big((a \sim b) + \text{M}\big)\Big) \\ \quad + \text{sin.v.}\Big(180^\circ - \big((a \sim b) \sim \text{M}\big)\Big) - 4. \end{cases}$$

4ome Formule.

$$\text{susin. v. D} = \begin{cases} \text{susin. v.}(\text{A} \sim \text{H}) + \text{sin. v.}\big[180^\circ \sim (d + \text{M})\big] \\ \quad + \text{sin.v.}\big(180^\circ - (d \sim \text{M})\big) + \text{sin.v.}\big((a \sim b) + \text{M}\big) \\ \quad + \text{sin.v.}\big((a \sim b) \sim \text{M}\big) - 4. \end{cases}$$

Je ne m'arrêterai pas à d'autres transformations qu'on pourroit faire; celles qui viennent d'être établies étant suffisantes pour l'objet que je me suis proposé.

Méthodes d'Approximation.

L'expression du cosinus de la distance vraie en termes des données du problème est, comme nous avons vu ci-dessus,

$$\cos. D = \frac{\cos. d \cos. A \cos. H}{\cos. a \cos. b} - \tan. a \tan. b \cos. A \cos. H + \sin. A \sin. H.$$

Représentons par u la parallaxe moins la réfraction en hauteur de la lune, par v la réfraction moins la parallaxe en hauteur du soleil, ou la simple réfraction de l'étoile, et par δ la correction totale de la distance apparente, telle que $D = d + \delta$.

Il s'agit à présent de trouver la valeur de δ en termes des corrections u, v, et de la distance et des hauteurs apparentes.

On a $A = a + u$, et $H = b - v$; et par conséquent

$$\begin{aligned}
\sin. A &= \sin. a \cos. u + \cos. a \sin. u \\
\cos. A &= \cos. a \cos. u - \sin. a \sin. u \\
\sin. H &= \sin. b \cos. v - \cos. b \sin. v \\
\cos. H &= \cos. b \cos. v + \sin. b \sin. v
\end{aligned}$$

d'où l'on déduit

$$\sin. A \sin. H = \begin{cases} \sin. a \sin. b \cos. u \cos. v + \cos. a \sin. b \sin. u \cos. v \\ - \sin. a \cos. b \cos. u \sin. v - \cos. a \cos. b \sin. u \sin. v \end{cases}$$

$$\cos. A \cos. H = \begin{cases} \cos. a \cos. b \cos. u \cos. v - \sin. a \cos. b \sin. u \cos. v \\ + \cos. a \sin. b \cos. u \sin. v - \sin. a \sin. b \sin. u \sin. v \end{cases}$$

Pour obtenir toute l'exactitude nécessaire, il suffira de porter les approximations jusqu'aux produits du second ordre, ou de deux dimensions des petits élémens u, v, δ. Or, un petit arc et son sinus ne différant entre eux que d'un produit du troisième ordre, on pourra prendre u pour sin. u, et v pour sin. v; mais, comme la différence entre le rayon et le cosinus d'un petit arc va jusqu'au second ordre, on devra substituer $1 - \frac{1}{2}u^2 = \cos. u$, et $1 - \frac{1}{2}v^2 = \cos. v$. En y introduisant ces valeurs, on aura,

en négligeant les produits de trois dimensions de u, v (ce que nous ferons aussi par la suite),

$$\text{sin. A sin. H} = \begin{cases} \text{sin. } a \text{ sin. } b + u \text{ cos. } a \text{ sin. } b - v \text{ sin. } a \text{ cos. } b \\ -uv \text{ cos. } a \text{ cos. } b - \frac{1}{2}u^2 \text{sin. } a \text{ sin. } b - \frac{1}{2}v^2 \text{sin. } a \text{ sin. } b. \end{cases}$$

$$\text{cos. A cos. H} = \begin{cases} \text{cos. } a \text{ cos. } b - u \text{ sin. } a \text{ cos. } b + v \text{ cos. } a \text{ sin. } b \\ -uv \text{ sin. } a \text{ sin. } b - \frac{1}{2}u^2 \text{cos. } a \text{ cos. } b - \frac{1}{2}v^2 \text{cos. } a \text{ cos. } b. \end{cases}$$

et, en substituant ces valeurs dans l'expression de cos. D, et faisant les réductions nécessaires, il résultera

$$\text{cos. D} = \begin{cases} \text{cos. } d + \frac{u \text{ sin. } b}{\text{cos. } a} - u \text{ cos. } d \text{ tan. } a - \frac{v \text{ sin. } a}{\text{cos. } b} + v \text{ cos. } d \text{ tan. } b \\ + \frac{uv(\text{sin.}^2 a - \text{cos.}^2 b - \text{cos. } d \text{ sin. } a \text{ sin. } b)}{\text{cos. } a \text{ cos. } b} - \frac{1}{2}u^2 \text{cos. } d - \frac{1}{2}v^2 \text{cos. } d. \end{cases}$$

Reprenons à présent $D = d + \delta$, et l'on aura

$\text{cos. D} = \text{cos. } d \text{ cos. } \delta - \text{sin. } d \text{ sin. } \delta$, ou (parceque $\text{cos. } \delta = 1 - \frac{1}{2}\delta^2$),

$\text{cos. D} = \text{cos. } d - \delta \text{ sin. } d - \frac{1}{2}\delta^2 \text{cos. } d$, ce qui, étant substitué dans l'équation précédente, donne

$$\delta = \begin{cases} -\frac{u \text{ sin. } b}{\text{cos. } a \text{ sin. } d} + u \text{ cot. } d \text{ tan. } a + \frac{v \text{ sin. } a}{\text{cos. } b \text{ sin. } d} - v \text{ cot. } d \text{ tan. } b \\ + \frac{uv(\text{sin.}^2 a - \text{cos.}^2 b + \text{cos. } d \text{ sin. } a \text{ sin. } b)}{\text{sin. } d \text{ cos. } a \text{ cos. } b} + \frac{1}{2}u^2 \text{cot. } d + \frac{1}{2}v^2 \text{cot. } d \\ - \frac{1}{2}\delta^2 \text{ cot. } d. \end{cases}$$

c'est-à-dire,

$$\delta = \begin{cases} -u\left(\frac{\text{sin. } b - \text{cos. } d \text{ sin. } a}{\text{sin. } d \text{ cos. } a}\right) + v\left(\frac{\text{sin. } a - \text{cos. } d \text{ sin. } b}{\text{sin. } d \text{ cos. } b}\right) \\ + uv\left(\frac{\text{cos. } d \text{ sin. } a \text{ sin. } b - \text{sin.}^2 a + \text{cos.}^2 b}{\text{sin. } d \text{ cos. } a \text{ cos. } b}\right) + \frac{1}{2}u^2 \text{cot. } d \\ + \frac{1}{2}v^2 \text{cot. } d - \frac{1}{2}\delta^2 \text{cot. } d. \end{cases}$$

Mais, on voit par cette même formule que (en continuant de négliger les produits de deux dimensions de u, v, δ), l'on a

$$\delta^2 = \begin{cases} u^2\left(\frac{\text{sin. } b - \text{cos. } d \text{ sin. } a}{\text{sin. } d \text{ cos. } a}\right)^2 + v^2\left(\frac{\text{sin. } a - \text{cos. } d \text{ sin. } b}{\text{sin. } d \text{ cos. } b}\right)^2 \\ - 2uv\left(\frac{\text{sin. } b - \text{cos. } d \text{ sin. } a}{\text{sin. } d \text{ cos. } a}\right)\left(\frac{\text{sin. } a - \text{cos. } d \text{ sin. } b}{\text{sin. } d \text{ cos. } b}\right) \end{cases}$$

donc, en substituant dans la formule précédente, il résultera

$$\delta = \begin{cases} -u\left(\frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a}\right) + v\left(\frac{\sin. a - \cos. d \sin. b}{\sin. d \cos. b}\right) \\ +uv\left(\frac{2\cos. d \sin. a \sin. b + \sin.^2 d - \sin.^2 a - \sin.^2 b}{\sin.^2 d \cos. a \cos. b}\right) \\ +\frac{1}{2}u^2 \cot. d\left(1 - \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a}\right)^2\right) \\ +\frac{1}{2}v^2 \cot. d\left(1 - \left(\frac{\sin. a - \cos. d \sin. b}{\sin. d \cos. b}\right)^2\right) \end{cases}$$

ou bien,

$$\delta = \begin{cases} -u\left(\frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a}\right) + v\left(\frac{\sin. a - \cos. d \sin. b}{\sin. d \cos. b}\right) \\ +uv\left(\frac{2\cos. d \sin. a \sin. b + \sin.^2 d - \sin.^2 a - \sin.^2 b}{\sin.^2 d \cos. a \cos. b}\right) \\ +\frac{1}{2}u^2 \cot. d\left(\frac{2\cos. d \sin. a \sin. b + \sin.^2 d - \sin.^2 a - \sin.^2 b}{\sin.^2 d \cos.^2 a}\right) \\ +\frac{1}{2}v^2 \cot. d\left(\frac{2\cos. d \sin. a \sin. b + \sin.^2 d - \sin.^2 a - \sin.^2 b}{\sin.^2 d \cos.^2 b}\right). \end{cases}$$

Voilà la formule qui exprime généralement les corrections qu'on doit appliquer à la distance apparente d, pour avoir la distance vraie D, ayant égard à toutes les équations qui dérivent de u, v, et des produits du second ordre de ces élémens. On peut, à son aide, prouver l'exactitude d'une méthode d'approximation quelconque. Il ne faut, pour cela, que transformer les expressions des corrections proposées, de manière à les mettre toutes en termes de la distance apparente, des hauteurs apparentes, et des corrections des hauteurs; et les comparer ainsi aux précédentes. Ce procédé m'a été fort utile pour examiner différentes méthodes, et découvrir leurs erreurs: mais je ne m'arrêterai pas, à présent, à ces détails; et pour donner un exemple de l'application de ma formule, je me bornerai à la considération de la solution du Dr. MASKELYNE.

Soient un arc M, tel que $\tan. M = \tan. \frac{1}{2}(a \sim b) \cot. \frac{1}{2}(a + b)$ (c'est le premier arc des préceptes de l'auteur), et un autre

arc N, tel que tan. N = tan. M cot. $\frac{1}{2}d$ (c'est le second arc des dits préceptes), et exprimons par R la réfraction qui convient à la hauteur de 45°. La correction totale qu'on doit appliquer à la distance par rapport aux réfractions des deux astres est, selon la méthode dont il s'agit, $= \frac{2\,R \text{ tan. } 2\,M}{\text{sin. } 2\,N}$, ou (parceque, en représentant la réfraction en hauteur de l'étoile par r', on a $R = r' \text{ tan. } b$) $= \frac{2\,r' \text{ tan. } b \text{ tan. } 2\,M}{\text{sin. } 2\,N}$. Réduisons l'expression $\frac{2\,R \text{ tan. } 2M}{\text{sin. } 2\,N}$ aux termes dont nous avons besoin.

En substituant tan. $2\,M = \frac{2 \text{ tan. } M}{1 - \text{tan.}^2 M}$, et sin. $2\,N = \frac{2 \text{ tan. } N}{1 + \text{tan.}^2 N}$ on aura $\frac{2\,R \text{ tan. } M\,(1 + \text{tan.}^2 N)}{\text{tan. } N\,(1 - \text{tan.}^2 M)}$. Mettons y tan. N = tan. M cot. $\frac{1}{2}d$, et il résultera $\frac{2\,R\,(1 + \text{tan.}^2 M \text{ cot.}^2 \frac{1}{2}d)}{\text{cot. } \frac{1}{2}d\,(1 - \text{tan.}^2 M)}$, qui, en substituant tan. M = tan. $\frac{1}{2}\,(a \sim b)$ cot. $\frac{1}{2}\,(a + b)$, se convertit en

$$\frac{2R\left(1+\text{tan.}^2\frac{1}{2}(a\sim b)\text{cot.}^2\frac{1}{2}(a+b)\text{cot.}^2\frac{1}{2}d\right)}{\text{cot.}\frac{1}{2}d\left(1-\text{tan.}^2\frac{1}{2}(a\sim b)\,\text{cot.}^2\frac{1}{2}(a+b)\right)} = \frac{2\,R\left(\text{cos.}^2\frac{1}{2}(a\sim b)\text{sin.}^2\frac{1}{2}(a+b)+\text{sin.}^2\frac{1}{2}(a\sim b)\text{cos.}^2\frac{1}{2}(a+b)\text{cot.}^2\frac{1}{2}d\right)}{\text{cot.}\frac{1}{2}d\left(\text{cos.}^2\frac{1}{2}(a\sim b)\text{sin.}^2\frac{1}{2}(a+b)-\text{sin.}^2\frac{1}{2}(a\sim b)\text{cos.}^2\frac{1}{2}(a+b)\right)}$$

ce qui, en substituant

cos.$^2\frac{1}{2}\,(a \sim b)$ sin.$^2\frac{1}{2}\,(a + b) = \frac{1}{4}\,(\text{sin.}^2 a + \text{sin.}^2 b + 2 \text{ sin. } a \text{ sin. } b)$

et

sin.$^2\frac{1}{2}\,(a \sim b)$ cos.$^2\frac{1}{2}\,(a + b) = \frac{1}{4}\,(\text{sin.}^2 a + \text{sin.}^2 b - 2 \text{ sin. } a \text{ sin. } b)$,

et, faisant les réductions nécessaires, donne

$$\frac{R\left((\text{sin.}^2 a + \text{sin.}^2 b + 2 \text{ sin. } a \text{ sin. } b)\,\text{sin.}^2\frac{1}{2}d + (\text{sin.}^2 a + \text{sin.}^2 b - 2 \text{ sin. } a \text{ sin. } b)\,\text{cot.}^2\frac{1}{2}d\right)}{2 \text{ sin. } \frac{1}{2}d \text{ cos. } \frac{1}{2}d \text{ sin. } a \text{ sin. } b}$$

d'où, en mettant $\frac{1}{2} - \frac{1}{2}\cos. d = \sin.^2\frac{1}{2}d$, et $\frac{1}{2} + \frac{1}{2}\cos. d = \cos.^2\frac{1}{2}d$, on tire $\frac{R\,(\text{sin.}^2 a + \text{sin.}^2 b - 2 \text{ cos. } d \text{ sin. } a \text{ sin. } b)}{\text{sin. } d \text{ sin. } a \text{ sin. } b}$.

Cette formule se résout en deux expressions, ou parties,

$\frac{R\,(\text{sin. } a - \text{cos. } d \text{ sin. } b)}{\text{sin. } d \text{ sin. } b}$, et $\frac{R\,(\text{sin. } b - \text{cos. } d \text{ sin. } a)}{\text{sin. } d \text{ sin. } a}$.

Appellons r la réfraction en hauteur de la lune. On aura

par la loi des réfractions* $R = r' \tan. b$, ou $R = r \tan. a$. Mettant ces valeurs dans les équations précédentes, elles se réduiront à $\frac{r' (\sin. a - \cos. d \sin. b)}{\sin. d \cos. b}$, et $\frac{r (\sin. b - \cos. d \sin. a)}{\sin. d \cos. a}$; qui expriment les corrections dépendantes de la réfraction de chaque astre.

Soit Q un arc $= N \approx \frac{1}{2} d$ (c'est le troisième arc des préceptes; le signe supérieur quand la hauteur du soleil ou de l'étoile est plus grande que celle de la lune, le signe inférieur dans le cas contraire) et représentons par P la parallaxe† horizontale de la lune. La correction de la distance relative à la parallaxe est, d'après le Dr. MASKELYNE, $= P \sin. a \tan. Q$.

On a $\tan. Q = \tan. (N \approx \frac{1}{2} d) = \frac{\tan. N \approx \tan. \frac{1}{2} d}{1 \approx \tan. N \tan. \frac{1}{2} d}$. Mais $\tan. N = \tan. \frac{1}{2} (a \sim b) \cot. \frac{1}{2} (a + b) \cot. \frac{1}{2} d$, ou . . . . . $\tan. N = \frac{(\sin. a \sim \sin. b) \cot. \frac{1}{2} d}{\sin. a + \sin. b}$. Donc, en substituant et en faisant les réductions nécessaires, on déduira

$$\tan. Q = \frac{(1 + \cos. d) (\sin. a \sim \sin. b) \approx (1 - \cos. d) (\sin. a + \sin. b)}{2 \sin. d \sin. a}$$

c'est-à-dire, $\tan. Q = \frac{\sin. b - \cos. d \sin. a}{\sin. d \sin. a}$.

Ainsi, la correction relative à la parallaxe est

$$\frac{P \sin. a (\sin. b - \cos. d \sin. a)}{\sin. d \sin. a} = \frac{P (\sin. b - \cos. d \sin. a)}{\sin. d}$$

* Le Dr. MASKELYNE ne néglige pas d'avoir égard aux corrections que demande cette supposition, parceque la loi des réfractions est un peu différente; ce que l'auteur fait par un procédé très simple, qu'il facilite par le moyen des deux Tables subsidiaires.

† C'est à P que le Dr. MASKELYNE applique une équation pour compenser la petite erreur qui résulte de la loi des réfractions adoptée auparavant. Ainsi, au lieu de la parallaxe horizontale, il emploie ce qu'il appelle la *parallaxe horizontale corrigée*.

ce qui, en représentant la parallaxe en hauteur par p, et substituant $P = \frac{p}{\cos. a}$, se réduit à $\frac{p(\sin. b - \cos. d \sin. a)}{\sin. d \cos. a}$.

On voit, par les équations ci-dessus, que la correction composée de la parallaxe et de la réfraction de la lune est $\frac{p(\sin. b - \cos. d \sin. a)}{\sin. d \cos. a} - \frac{r(\sin. b - \cos. d \sin. a)}{\sin. d \cos. a} = \frac{(p-r)(\sin. b - \cos. d \sin. a)}{\sin. d \cos. a}$. Expression identique à celle que nous avons trouvé relativement à u. L'expression $\frac{r'(\sin. a - \cos. d \sin. b)}{\sin. d \cos. b}$ convient aussi avec celle qui dérive de v. Il nous reste à examiner les corrections relatives aux produits du second ordre.

En faisant tan. Q tan. a = cos. S (S est le quatrième arc des dits préceptes), on a pour la troisième correction du Dr. Maskelyne* $\frac{\left(P - \frac{r}{\cos. a}\right)^2 \cos.^2 a \sin.^2 S}{2 \tan. d}$. Cette expression se convertit en $\frac{(P \cos. a - r)^2 \sin.^2 S}{2 \tan. d} = \frac{(p-r)^2 \sin.^2 S}{2 \tan. d}$.

En prenant la valeur de tan. Q établie ci-dessus, on a cos. S $= \frac{(\sin. b - \cos. d \sin. a) \tan. a}{\sin. d \sin. a} = \frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a}$, d'où l'on déduit $\sin.^2 S = 1 - \cos.^2 S = \frac{2 \cos. d \sin. a \sin. b + \sin.^2 d - \sin.^2 a - \sin.^2 b}{\sin.^2 d \cos.^2 a}$; et, en substituant cette expression dans la précédente, il résultera, pour la correction dont il s'agit,

$$\frac{1}{2}(p-r)^2 \cot. d \left(\frac{2 \cos. d \sin. a \sin. b + \sin.^2 d - \sin.^2 a - \sin.^2 b}{\sin.^2 d \cos.^2 a}\right).$$

Expression identique à celle que nous avons trouvé relativement à u^2.

La quatrième correction est (en représentant la troisième par m), $= \frac{2 r' m}{\cos. d \cos. b \left(P - \frac{r}{\cos. a}\right)}$; qui en substituant la valeur de m,

* La quantité $P - \frac{r}{\cos. a}$ est ce que l'auteur appelle *parallaxe horizontale diminuée*.

et celle de $P = \frac{p}{\cos. q}$, se réduit à

$$r' (p - r) \left(\frac{2 \cos. d \sin. a \sin. b + \sin.^2 d - \sin.^2 a - \sin.^2 b}{\sin.^2 d \cos. a \cos. b} \right).$$

Expression identique à celle que nous avons trouvée relativement à $u\,v$.

La cinquième correction est $= \frac{m\, r'^3}{\cos.^3 b \left(P - \frac{r}{\cos. a} \right)}$, qui se réduit facilement à l'expression déduite ci-dessus relativement à v^3.

On voit, par cet examen, que la méthode en question a toute l'exactitude qu'on peut désirer; et c'est la raison qui m'a déterminé à la choisir, entre toutes celles que je connois, pour donner un example satisfaisant et complet de la manière d'employer mes formules dans ces sortes d'analyses.

Je finirai cet article en donnant quelques formules, qu'on pourra employer pour calculer les corrections qu'on doit appliquer à la distance apparente pour avoir la distance vraie.

Reprenons l'equation

$$\delta = \begin{cases} -u \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a} \right) + v \left(\frac{\sin. a - \cos. d \sin. b}{\sin. d \cos. b} \right) + uv \left(\frac{2 \cos. d \sin. a \sin. b + \sin.^2 d - \sin.^2 a - \sin.^2 b}{\sin.^2 d \cos. a \cos. b} \right) \\ + \frac{1}{2} u^2 \cot. d \left(1 - \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a} \right)^2 \right) + \frac{1}{2} v^2 \cot. d \left(1 - \left(\frac{\sin. a - \cos. d \sin. b}{\sin. d \cos. b} \right)^2 \right). \end{cases}$$

On voit facilement que

$$\frac{2 \cos. d \sin. a \sin. b + \sin.^2 d - \sin.^2 a - \sin.^2 b}{\sin.^2 d \cos. a \cos. b} = \sqrt{\left(1 - \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a} \right)^2 \right) \left(1 - \left(\frac{\sin. a - \cos. d \sin. b}{\sin. d \cos. b} \right)^2 \right)}$$

Donc, en substituant cette expression dans la formule précédente, elle se réduit à

$$\delta = \begin{cases} -u \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a} \right) + v \left(\frac{\sin. a - \cos. d \sin. b}{\sin. d \cos. b} \right) + \frac{uv}{\sin. d} \sqrt{\left(1 - \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a} \right)^2 \right) \left(1 - \left(\frac{\sin. a - \cos. d \sin. b}{\sin. d \cos. b} \right)^2 \right)} \\ + \frac{1}{2} u^2 \cot. d \left(1 - \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a} \right)^2 \right) + \frac{1}{2} v^2 \cot. d \left(1 - \left(\frac{\sin. a - \cos. d \sin. b}{\sin. d \cos. b} \right)^2 \right). \end{cases}$$

Faisons $\frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a} = \cos. L$, et $\frac{\sin. a - \cos. d \sin. b}{\sin. d \cos. b} = \cos. S$, (et l'on voit, que L représente l'angle formé par la distance apparente des deux astres et la distance apparente de la lune au zénith, et S l'angle formé de la même manière au lieu apparent du soleil ou de l'étoile), et la formule se convertira en

$$\delta = \begin{cases} -u \cos. L + v \cos. S + u v \text{ cosec. } d \sin. L \sin. S \\ + \frac{1}{2} u^2 \cot. d \sin.^2 L + \frac{1}{2} v^2 \cot. d \sin.^2 S. \end{cases}$$

Les principales corrections sont $-u \cos. L + v \cos. S$, ou $-u \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a}\right) + v \left(\frac{\sin. a - \cos. d \sin. b}{\sin. d \cos. b}\right)$. Appellons les Σ, et nous aurons

$$\Sigma = -u \cos. L + v \cos. S$$

$$\Sigma = -u + u \sin. v. L + v - v \sin. v. S$$

$$\Sigma = -u + u (1 - \cos. L) + v - v (1 - \cos. S)$$

$$\Sigma = -u + u \left(1 - \frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a}\right) + v - v \left(1 - \frac{\sin. a - \cos. d \sin. b}{\sin. d \cos. b}\right)$$

$$\Sigma = \begin{cases} -u + u \left(\frac{\sin. d \cos. a + \cos. d \sin. a - \sin. b}{\sin. d \cos. a}\right) \\ + v - v \left(\frac{\sin. d \cos. b + \cos. d \sin. b - \sin. a}{\sin. d \cos. b}\right) \end{cases}$$

$$\Sigma = -u + u \left(\frac{\sin. (d + a) - \sin. b}{\sin. d \cos. a}\right) + v - v \left(\frac{\sin. (d + b) - \sin. a}{\sin. d \cos. b}\right)$$

$$\Sigma = \begin{cases} -u + u \frac{2 \cos. \frac{1}{2} (d + a + b) \sin. \frac{1}{2} (d + a - b)}{\sin. d \cos. a} \\ + v - v \frac{2 \cos. \frac{1}{2} (d + a + b) \sin. \frac{1}{2} (d + b - a)}{\sin. d \cos. b} \end{cases}$$

L'application de ces formules n'exige aucune distinction de cas; car il faudra toujours ajouter à la distance apparente les quantités $v + u \frac{2 \cos. \frac{1}{2} (d + a + b) \sin. \frac{1}{2} (d + a - b)}{\sin. d \cos. a}$, et retrancher la somme $u + v \frac{2 \cos. \frac{1}{2} (d + a + b) \sin. \frac{1}{2} (d + b - a)}{\sin. d \cos. b}$. Les opérations sont

d'ailleurs assez faciles, car on n'a besoin de chercher que six logarithmes; le cosinus de $\frac{1}{2}(d+a+b)$ et le sinus de d se trouvant dans les deux expressions qu'on calcule.

Cette méthode me paroit utile, pour le calcul des deux corrections principales. Quant aux autres corrections, je me bornerai à indiquer quelques expressions nouvelles, qui dérivent des précédentes, sans y entrer dans les détails de leurs propriétés particulières.

Représentons la correction relative à u^s par m. Dans le calcul de la correction relative à u, l'on trouve le logarithme de sin.v.L. En faisant usage des tables des sinus-verses, on pourra, donc, déduire m par l'une des expressions $\frac{1}{2}u^s \cot. d \sin.^s L$, ou $\frac{1}{2}u^s \cot. d \sin. v. L. \sin. v. L.$

J'observerai ici que, comme cette formule contient le quarré de l'arc u^s, en parties de la circonférence, il faudra diviser dans le calcul par R″, c'est-à-dire, par la valeur du rayon ou du sinus total, en secondes, pour avoir l'équation aussi en secondes. La même remarque a lieu pour toutes les expressions semblables à la précédente.

Le logarithme de R″ est 5.3144251. Donc, pour le calcul de m par la formule précédente, on pourra se servir du logarithme constant négatif $5.3144251 + 0.3010300 = 5.6154551$, ou, ce qui revient au même, du logarithme constant positif 4.3845449.

Si l'on emploie les logarithmes logistiques, ou proportionels pour 3^h ou 10800″, ces logarithmes étant réciproques, on aura pour logarithme constant positif

$$5.3144251 + 0.3010300 - 4.0334238 = 1.5820313.$$

En employant seulement les tables des sinus, on pourra prendre la moitié des quatre logarithmes $\frac{\cos.\frac{1}{2}(d+a+b)\sin.\frac{1}{2}(d+b-a)}{\sin. d \cos. b}$

dans le calcul de la première correction, ce qui donne log. sin. $\frac{1}{2}$L et trouver ensuite m par l'expression $2u^2 \cot. d \sin.^2 \frac{1}{2}\text{L} \cos.^2 \frac{1}{2}\text{L}$.

Pour le calcul de cette formule, on aura le logarithme constant positif 4.9866049, en employant les logarithmes ordinaires, et 0.9799713, en employant les logarithmes proportionels.

Les formules que nous avons établies fournissent une autre méthode pour déterminer m.

En reprenant $m = \frac{1}{2} u^2 \cot. d \text{ sin. v. L susin. v. L}$, et substituant $2 - \text{sin. v. L} = \text{susin. v. L}$, on déduit

$$m = u^2 \cot. d \text{ sin. v. L} - \tfrac{1}{2} u^2 \cot. d \text{ sin. v.}^2 \text{L}$$

Or, u sin. v. L n'est autre chose que l'équation . . . . . $u \frac{2 \cos. \frac{1}{2}(d+a+b) \sin. \frac{1}{2}(d+a-b)}{\sin. d \cos. a}$, qu'on calcule pour la correction principale relative à u; donc, en représentant cette équation par μ, on aura

$$m = u\,\mu \cot. d - \tfrac{1}{2}\mu^2 \cot. d$$

ou

$$m = \mu\,(u - \tfrac{1}{2}\mu) \cot. d.$$

Cette manière de calculer m est très commode, et je crois qu'on doit surtout la préférer, quand on se bornera à ce dégré d'approximation, qui sera suffisant dans la plupart des circonstances, en négligeant les équations relatives à uv et v^2.

Pour le calcul de $\mu\,(u - \frac{1}{2}\mu) \cot. d$, ou de $u^2 \cot. d \sin. \text{v. L}$, on aura le logarithme constant positif 4.6855749, en employant les logarithmes ordinaires, et 1.2810013, en employant les logarithmes proportionels. Pour le calcul de $\frac{1}{2}\mu^2 \cot. d$ le logarithme constant positif est 4.3845449 ou 1.5820313.

Représentons par n la correction relative à v^2, et l'on aura de même les expressions suivantes.

$$n = \tfrac{1}{2} v^2 \cot. d \text{ sin. v. S susin. v. S}$$

$$n = 2 v^2 \cot. d \sin.^2 \tfrac{1}{2}\text{S} \cos.^2 \tfrac{1}{2}\text{S}.$$

Et, en représentant par π l'équation principale relative à v,

c'est-a-dire, $v \frac{2\cos.\frac{1}{2}(d+a+b)\sin.\frac{1}{2}(d+b-a)}{\sin.d\cos.b}$, on déduira aussi

$$n = v\pi \cot. d - \tfrac{1}{2}\pi^3 \cot. d$$

ou $$n = \pi\left(v - \tfrac{1}{2}\pi\right)\cot. d.$$

Pour les logarithmes constans, qui conviennent à ces formules, je m'en rapporte à ce que j'ai dit au sujet des corrections relatives à u^2.

Quant à la correction relative à $u\,v$, que nous appellerons ω, on a $$\omega = \frac{uv\sin.L\sin.S}{\sin.d}.$$

Ayant recours aux corrections précédentes, on voit que $u\,v\sin.L\sin.S = 2\tan.d\sqrt{mn}$; donc, en substituant, il résultera $$\omega = \frac{2\sqrt{mn}}{\cos.d}.$$

De l'expression qui précède, l'on tire celle-ci

$$\omega = \frac{2\sqrt{\alpha\gamma\left(\alpha-\frac{1}{2}\mu\right)\left(\gamma-\frac{1}{2}\pi\right)}}{\sin.d}.$$

De l'expression trouvée $\omega = \frac{uv\sin.L.\sin.S}{\sin.d}$, qu'on pourra employer, quand on calculera les autres corrections par les sinus-verses, on déduit $\omega = \frac{4uv\sin.\frac{1}{2}L\cos.\frac{1}{2}L\sin.\frac{1}{2}S\cos.\frac{1}{2}S}{\sin.d}$, dont on pourra faire usage, quand on calculera seulement par les sinus.

Nous remarquerons, qu'en se bornant à la correction relative à u^2, et négligeant les autres équations qui dépendent des produits de deux dimensions, comme l'on pratique dans quelques méthodes connues, on pourra faire le calcul des deux corrections principales par les formules précédentes, et puis trouver la troisième correction de la manière adoptée par Mr. LYONS, en se servant de la Table XIII. des *Requisite Tables* de 1781. (Mr. LYONS avoit donné cette Table dans l'édition de 1767) Ce procédé seroit très commode et assez exact pour les cas ordinaires, où l'on se contente des méthodes qui ne sont pas rigoureuses. Au reste, le calcul de cette correction par la der-

nière formule que nous avons donnée, est presque aussi commode, et réunit outre cela l'avantage de ne pas demander des tables subsidiaires.

Remarques générales sur les Méthodes précédentes.

Les méthodes directes par les logarithmes, au moins, les meilleures de cette espèce, procurent la distance réduite, avec exactitude, et suivant des règles constantes. Les opérations sont, d'ailleurs, assez simples, et n'exigent pas un grand nombre de logarithmes. Mais ces avantages se trouvent diminués dans la pratique. En effet, on est obligé d'employer les logarithmes avec plusieurs décimales, ce qui augmente la masse du calcul dans une certaine proportion, et produit d'autres inconvéniens; car on ne peut pas se dispenser de calculer et d'appliquer des parties proportionelles, quand on fait usage des tables ordinaires, et si, pour éviter cette peine, on a recours aux tables qui donnent les logarithmes de seconde en seconde, la facilité qu'elles offrent est moins considérable qu'on ne pourroit le penser, par l'embarras d'un gros volume, où l'on ne laisse pas de perdre du tems à feuilleter, pour trouver l'endroit qu'on cherche.

Les propriétés caractéristiques des méthodes d'approximation sont différentes. Elles sont indirectes, et demandent plus ou moins de distinctions des cas. Les opérations sont, outre cela, longues et complexes, surtout quand on veut arriver à un résultat exact. En revanche, comme ce qu'on calcule n'est pas le total de la distance vraie, mais seulement les corrections qu'on doit appliquer à la distance apparente (quantités qui ne sont pas très considérables), il suffit d'employer les logarithmes avec peu de décimales; et de cette manière on peut faire le calcul avec des tables très courtes, et négliger les parties proportionelles.

Les méthodes par les sinus-verses naturels me paroissent réunir les avantages des deux sortes de procédés que nous venons de considérer, sans être sujettes à leurs inconvéniens. Mais comme l'utilité de ces méthodes dépend des tables que j'ai calculées, et que j'ai annoncées depuis plusieurs années, mais qui ne sont pas encore connues, je crois superflu d'ajouter plus de réflexions à ce sujet; m'en rapportant là-dessus à ce qu'on trouvera dans cet ouvrage, actuellement sous presse, et qui est destiné à faciliter les opérations pratiques.

Méthode pour avoir égard à la Figure elliptique de la Terre.

La figure elliptique de la Terre peut influer de deux manières dans la réduction des distances lunaires. La 1re, en ce que les éphémérides donnant la parallaxe horizontale de la lune pour un lieu particulier du globe, si on l'emploie pour un autre lieu, on commet une erreur qui dépend de la différence des parallaxes qui conviennent aux deux latitudes. La 2e, en ce que la verticale hors de l'équateur et des poles n'aboutissant pas au centre de la Terre, les hauteurs observées ne sont pas celles qu'on prendroit si le globe étoit sphérique. On remédieroit à la première cause, en appliquant à la parallaxe horizontale de la lune tirée des éphémérides, l'équation nécessaire pour la réduire à la situation actuelle du vaisseau. Pour la seconde cause, l'on pourroit appliquer à chaque hauteur observée la correction convénable, qui est égale à l'angle formé par la verticale et le rayon terrestre, multiplié par le cosinus de l'azimuth de l'astre. Mais ce procédé seroit embarrassant, et peu précis; car il exige qu'on prenne les azimuths de la lune et du soleil, en même tems qu'on observe leur dis-

tance, et les azimuths donnés par le compas doivent en général être très fautifs. Nous chercherons, donc, des formules pour arriver au même but seulement par le calcul.

Entre les équations précédentes, il n'y a que celle qui dépend de u, où l'influence des causes mentionnées mérite d'être considérée: car la correction v est ordinairement trop petite pour y avoir égard. Nous pourrons aussi négliger dans u la réfraction, en nous bornant à la parallaxe, qui est l'élément le plus considérable. Ainsi l'équation $-u\left(\frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a}\right)$, se réduit à $-p\left(\frac{\sin. b - \cos. d \sin. a}{\sin. d \cos. a}\right)$, ou $-\mathrm{P}\left(\frac{\sin. b - \cos. d \sin. a}{\sin. d}\right)$.

Supposons que P est la parallaxe horizontale équatoriale, et différentions en supposant P, a, b variables. On aura, en considérant que la différentielle de P est constamment négative,

$$\partial \mathrm{P}\left(\frac{\sin. b - \cos. d \sin. a}{\sin. d}\right) - \mathrm{P}\left(\frac{\partial b \cos. b - \partial a \cos. d \cos. a}{\sin. d}\right).$$

Ce sont les corrections qu'on doit appliquer à la distance vraie calculée par les méthodes ordinaires, où $\partial \mathrm{P}$ exprime la différence entre la parallaxe équatoriale et celle qui convient à la latitude du lieu de l'observation. Il s'agit à présent de déduire des formules propres pour le calcul.

Représentons l'azimuth de la lune (compté depuis le quart de méridien où se trouve le pole élevé) par F, sa déclinaison par B; l'azimuth du soleil ou de l'étoile (comptés de la même manière) par f, sa déclinaison par b; et l'angle de la verticale et du rayon terrestre pour le lieu de l'obsérvation par n. Nous aurons

$$\partial a = -n \cos. \mathrm{F} = -n\left(\frac{\sin. \mathrm{B} - \sin. l \sin. a}{\cos. l \cos. a}\right)$$

et

$$\partial b = -n \cos. f = -n\left(\frac{\sin. b - \sin. l \sin. b}{\cos. l \cos. b}\right)$$

Substituant ces expressions, et faisant la somme des corrections $= x$, on déduira

$$x = \delta P\left(\frac{\sin. b - \cos. d \sin. a}{\sin. d}\right) + Pn\left(\frac{\sin. b \cos. b - \sin. l \sin. b \cos. b}{\sin. d \cos. l \cos. b} - \frac{\sin. B \cos. d \cos. a - \sin. l \cos. d \sin. a \cos. a}{\sin. d \cos. l \cos. a}\right)$$

$$x = \delta P\left(\frac{\sin. b - \cos. d \sin. a}{\sin. d}\right) + Pn\left(\frac{\sin. b - \sin. l \sin. b - \sin. B \cos. d + \sin. l \cos. d \sin. a}{\sin. d \cos. l}\right)$$

$$x = \delta P\left(\frac{\sin. b - \cos. d \sin. a}{\sin. d}\right) - Pn \tan. l \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d}\right) + Pn\left(\frac{\sin. b - \sin. B \cos. d}{\sin. d \cos. l}\right)$$

$$x = (\delta P - Pn \tan. l)\left(\frac{\sin. b - \cos. d \sin. a}{\sin. d}\right) + Pn\left(\frac{\sin. b - \sin. B \cos. d}{\sin. d \cos. l}\right).$$

Représentons l'applatissement de la Terre par e, en supposant le demi-diametre de l'équateur $= 1$, et nous aurons $\delta P = P e \sin.^2 l$, et $n = 2 e \sin. l \cos. l$;* ce qui, étant substitué, donne

$$x = (\delta P - 2 \delta P \tan. l \cot. l)\left(\frac{\sin. b - \cos. d \sin. a}{\sin. d}\right) + 2 P e \sin. l \left(\frac{\sin. b - \sin. B \cos. d}{\sin. d}\right)$$

$$x = -\delta P\left(\frac{\sin. b - \cos. d \sin. a}{\sin. d}\right) + 2 P e \sin. l \left(\frac{\sin. b - \sin. B \cos. d}{\sin. d}\right).$$

* Voici la démonstration.

Représentons le demi-axe terrestre par b, supposant le demi-diametre de l'équateur $= 1$, et l'applatissement $1 - b = e$. Et soit, pour un lieu particulier, l la latitude, r le rayon terrestre, c l'angle formé au centre de la terre par le rayon et le demi-diametre de l'équateur, et x l'angle de la verticale et du rayon terrestre.

On aura (*Voyez* la *Trigonométrie* de Mr. CAGNOLI) $\tan. c = b^2 \tan. l$. Faisons aussi $\tan. z = b \tan. l$.

On déduira $r^2 = \frac{\cos.^2 z}{\cos.^2 c} = \frac{\sec.^2 c}{\sec.^2 z} = \frac{1 + b^4 \tan.^2 l}{1 + b^2 \tan.^2 l}$. Mais on a, à très peu près, $b^2 = 1 - 2e$, $b^4 = 1 - 4e$, et $1 - r^2 = 2 - 2r$; donc, en substituant, on tirera $2 - 2r = 1 - \frac{1 + b^4 \tan.^2 l}{1 + b^2 \tan.^2 l}$, et $2r = 1 + \frac{1 + b^4 \tan.^2 l}{1 + b^2 \tan.^2 l}$, qui se réduit à $2r = \frac{2 \tan.^2 l}{1 + \tan.^2 l} = \frac{2 \tan.^2 l}{\sec.^2 l} = 2 \cos.^2 l$. Ainsi $1 - r = e \sin.^2 l$, et par conséquent $P - Pr = Pe \sin.^2 l$.

L'angle x est $= l - c$. Par conséquent $\tan. x = \frac{\tan. l - \tan. c}{1 + \tan. l \tan. c} = \frac{\tan. l - b^2 \tan. l}{1 + b^2 \tan.^2 l}$, et substituant $b^2 = 1 - 2e$, on aura $\tan. x = \frac{2e \tan. l}{1 + \tan.^2 l - 2e \tan.^2 l}$, qui se réduit à $\tan. x = \frac{2e \tan. l}{\sec.^2 l} = 2e \sin. l \cos. l$, d'où, parceque x est toujours petit, il résulte $x = 2e \sin. l \cos. l$.

Or, si, dans la réduction de la distance, l'on emploie la parallaxe horizontale pour l'équateur, augmentée de la différence ∂P entre cette parallaxe et celle qui convient à la latitude du lieu de l'observation, la distance vraie, ainsi calculée, se trouvera corrigée de la quantité $\partial P \left(\frac{\sin. b - \cos. d \sin. a}{\sin. d}\right)$; car l'équation dépendante de cet élément, sera alors $-(P + \partial P)\left(\frac{\sin. b - \cos. d \sin. a}{\sin. d}\right)$. On pourra, donc, employer la parallaxe préparée de cette manière;* ce qu'on pourra faire très facilement, car si les éphémérides donnent la parallaxe pour un lieu particulier, il suffira d'y ajouter l'équation relative à la latitude de ce lieu, ainsi que l'équation relative au lieu de l'observation.

A la distance obtenue, l'on devra appliquer la quantité $2 P e \sin. l \left(\frac{\sin. b - \sin. B \cos. d}{\sin. d}\right)$. Soit cette équation $= \epsilon$, et nous aurons

$$\epsilon = 2 P e \sin. l \cos. B \left(\frac{\sin. b - \sin. B \cos. d}{\cos. B \sin. d}\right)$$

$$\epsilon = 2 P e \sin. l \cos. B \left(1 - 1 + \frac{\sin. b - \sin. B \cos. d}{\cos. B \sin. d}\right)$$

$$\epsilon = 2 P e \sin. l \cos. B - 2 P e \sin. l \cos. B \left(1 + \frac{\sin. B \cos. d - \sin. b}{\cos. B \sin. d}\right)$$

$$\epsilon = 2 P e \sin. l \cos. B - 2 P e \sin. l \left(\frac{\cos. B \sin. d + \sin. B \cos. d - \sin b}{\sin. d}\right)$$

$$\epsilon = 2 P e \sin. l \cos. B - 2 P e \sin. l \left(\frac{\sin. (d + B) - \sin. b}{\sin. d}\right)$$

$$\epsilon = 2 P e \sin. l \cos. B - 4 P e \sin. l \frac{\cos. \frac{1}{2}(d + B + b) \sin. \frac{1}{2}(d + B - b)}{\sin. d}$$

Cette expression a l'avantage de ne demander aucune distinction de cas, car on devra toujours ajouter à la distance cal-

* C'est ainsi que le savant Mr. DE BORDA le pratique, dans la méthode qu'il nous a donnée à ce sujet (voyez son *Traité du Cercle de Reflexion*), et que je n'ai pas manqué de consulter avant de travailler à la rédaction de cet article.

culée l'équation $2\,P\,e\,\sin.\,l\,\cos.\,B$, et retrancher l'équation $4\,P\,e\,\sin.\,l\,\frac{\cos.\frac{1}{2}(d+B+b)\,\sin.\frac{1}{2}(d+B-b)}{\sin.\,d}$; et le résultat restera, ainsi, depouillé des erreurs qui dépendent de l'applatissement de la Terre.

On pourra, dans ces opérations, employer toujours pour P la parallaxe horizontale moyenne, 57′, et l'on aura 1.32855 pour le logarithme constant de $2\,P\,e$, et 1.62958 pour le logarithme constant de $4\,P\,e$, en supposant l'applatissement $= \frac{1}{321}$.

Pour faciliter le calcul, j'ai construit deux tables, dont l'une donne l'équation $2\,P\,e\,\sin.\,l\,\cos.\,B$, et l'autre le logarithme de $4\,P\,e\,\sin.\,l$.

On pourroit aussi trouver

$$i = -2\,P\,e\,\sin.\,l\,\cos.\,B + 2\,P\,e\,\sin.\,l\,\cos.\,B\left(1+\frac{\sin.\,b-\sin.\,B\,\cos.\,d}{\cos.\,B\,\sin.\,d}\right)$$

$$i = -2\,P\,e\,\sin.\,l\,\cos.\,B + 2\,P\,e\,\sin.\,l\left(\frac{\sin.\,b+\cos.\,B\,\sin.\,d-\sin.\,B\,\cos.\,d}{\sin.\,d}\right)$$

$$i = -2\,P\,e\,\sin.\,l\,\cos.\,B + 2\,P\,e\,\sin.\,l\left(\frac{\sin.\,b+\sin.\,(d-B)}{\sin.\,d}\right)$$

$$i = -2\,P\,e\,\sin.\,l\,\cos.\,B + 4\,P\,e\,\sin.\,l\,\frac{\sin.\frac{1}{2}(b+d-B)\cos.\frac{1}{2}\left(b\sim(d-B)\right)}{\sin.\,d}$$

Si l'on préféroit d'employer les distances au pole élevé, au lieu des déclinaisons des astres, on auroit (en appellant B′, et b' les distances polaires correspondantes à B, et b)

$$i = -2\,P\,e\,\sin.\,l\,\sin.\,B' + 4\,P\,e\,\sin.\,l\,\frac{\sin.\frac{1}{2}(d+B'+b')\,\sin.\frac{1}{2}(d+B'-b')}{\sin.\,d}.$$

ou bien

$$i = 2\,P\,e\,\sin.\,l\,\sin.\,B' - 4\,P\,e\,\sin.\,l\,\frac{\sin.\frac{1}{2}\left(b'+(d\sim B')\right)\sin.\frac{1}{2}\left(b'-(d\sim B')\right)}{\sin.\,d}$$

APPENDICE.

Exemples des calculs de quelques unes des Solutions établies ci-dessus, par les Tables ordinaires.

EXEMPLE I.

Calcul de la Latitude du lieu par deux Hauteurs du Soleil, et l'Intervalle de Tems écoulé entre les Observations.

Observations faites dans l'hémisphère septentrional.

Hauteurs vraies ⊙		Demi-intervalle	Déclinaison ⊙	Distance polaire ⊙	
45° 5′ 42″		1h 30′ = 22° 30′	12° 0′ N	78° 0′	
5 36 6					
		L. cos. déclinaison	9.99040		
		L. sin. demi-intervalle	9.58284		
		Somme - -	9.57324	L. sin. A -	21° 58′ 56″
				Dist. polaire	78 0 0
		L. sin. - -	9.91867	Différence	56 1 4
2A - -	43° 57′ 52″	L. sin. demi-intervalle	9.58284		
Petite hauteur	5 36 6	C. l. sin - -	0.15851	2A - -	43 57 52
Grande hauteur	45 5 42	Somme - -	19.66002		
Somme	94 39 40	Demi-somme -	9.83001	L. sin. -	42 32 22
Demi-somme -	47 19 50	L. cos. - -	9.83108		
Différence -	2 14 8	L. sin. - -	8.59115		
		C. l. sin. 2 A -	0.15851		
		C. l. cos. petite hauteur	0.00208		
		Somme - -	18.58282		
		Demi-somme - -	9.29141	L. sin. -	11 16 51
Distance polaire	78 0 0	L. sin. - -	9.71509	Différence	31 15 31
Petite hauteur	5 36 6	Demi-l. cos. pet. haut.	4.99896		
Somme (+ 90°)	173 36 6	Demi-l. cos. declin.	4.99520		
Demi-somme	86 48 3	C. sin. - -	0.00068		
		L. sin. N (somme)	9.70993		
		L. cos. N - -	9.93375		
		Différence -	9.93307	L. sin. (B) -	59 0 3
		Latitude du lieu (2 B — 90°)	- -	-	28 0 6

EXEMPLE II.

Calcul de la Latitude du lieu par deux Hauteurs du Soleil, et l'Intervalle de Tems écoulé entre les Observations, ayant d'ailleurs la Latitude estimée.

En déduisant premierement l'Angle horaire moyen.

	Hauteurs vraies ⊙			Heures des observ.			Lat. estimée			Déclinaison ⊙		
1re Observation	30°	13′	14″	7h	32′	16″						
2de Observation	50	3	55	10	27	48	- - -	56°	29′ S	-	20°	6′ 40″ S
				2	55	32						
	Intervalle -			1	27	46 = 21° 56′ 30″						
	Demi-intervalle											
Grande hauteur -	50°	3′	55″									
Petite hauteur -	30	13	14				1re supposition.			2de supposition.		
Somme - -	80	17	9									
Demi-somme -	40	8	34	L. cos. - -			9 88334					
Différence - -	9	55	20	L. sin. - -			9.23631					
Demi-intervalle -	21	56	30	C. l. sin. -			0.42752					
Déclinaison - -	20	6	40	C. l. cos. -			0.02732					
				Somme - -			9.57449			-	-	9.57449
Latitude estimée (−30′)	55	59	0	C. l. cos. -			0.25225			+ 1°		0.26370
Horaire moyen -	42	8	47	L. sin. (somme)			9.82674			43°	32′ 50″	9.83819
Demi-intervalle -	21	56	30	- - - -			- - -			21	56 30	
Petit horaire - -	20	12	17	- - - -			- - -			21	36 20	
Demi-petit horaire -	10	6	8	C. l. sin. -			0.75596			10	48 10	0.72716
				Demi-c. l. cos. decl.			0.01366			-	-	0.01366
				Demi-c. l. cos. lat. -			0.12612			-	-	0.13185
Demi-(gr. haut. + 90°)	70	1	57	L. sin. - -			9 97308			-	-	9 97308
				L. tan. A (somme)			10.86882			-	-	10.84575
				L. sin. A -			9.99606			-	-	9.99503
Demi-dist. méridienne	18	28	30	L. cos. (différence)			9 97702			18	18 16	9 97745
Distance méridienne	36	57	0	- - - -			- - -			36	36 32	
Déclinaison - -	20	6	40	- - - -			- - -			20	6 40	
Latitude calculée -	57	4		- - - -			- - -			56	43	
Latitude supposée -	55	59		- - - -			- - -			56	59	
	1	5		-	Somme 1° 21′		- -			0	16	
Équation de la deuxième latitude supposée $\frac{16' \times 60'}{81'}$										0	12	
Latitude du lieu -				- - - -						56	47	

Remarque. Pour appliquer l'équation trouvée à l'une des latitudes calculées de la manière convenable, afin de déduire la latitude corrigée, on pourra consulter ce qui a été dit ci-dessus dans les pages 60, et 61.

EXEMPLE III.

Calcul de la Latitude du lieu par deux Hauteurs du Soleil, et l'Intervalle de Tems écoulé entre les Observations, ayant d'ailleurs la Latitude estimée.

En déduisant premièrement le grand Angle horaire.

Hauteurs vraies ⊙			Heures des observ.			Lat. estimée		Déclinaison ⊙		
1re Observation	68° 29′	50″	11h	30′	20″,5	39° 38′ N		20° 41′	33″	N
2de Observation	71 9	15	12	27	1			20	41	7
Intervalle			0	56	40,5 =			14	10	7,5
Différence en longitude contractée par le vaisseau entre les observations								0	7	0 à l'ouest
Intervalle preparé pour le calcul								14	3	7,5

				1re supposition.		2me supposition.
Petite hauteur	68°	29′	50″			
Latitude estimée (−30′)	39	8	0	C. l. cos.	+ 1°	0.11660
Distance polaire	69	18	27	C. l. sin.		0.02896
Somme	176	56	17			
Demi-somme	88	28	8	L. cos.	+ 30′	8.25516
Différence	19	58	18	L. sin.	+ 30′	9.54375
				Somme		17.94447
Demi-grand horaire	6	26	20	L. sin. (demi-somme)	5° 22′ 57″	8.97223
Demi-intervalle	7	1	34		7 1 34	
Demi-petit horaire	0	35	14	C. l. sin.	1 38 37	1.54238
				Demi-c. l. cos. lat.		0.05830
				Demi-c. l. sin. dist. p.		0.01448
Demi-(gr. haut. + 90°)	80	34	37	L. sin.		9.99410
				L. tan. A (somme)		11.60926
				L. sin. A		9.99998
Demi-dist. méridienne	9	24	25	L. cos. (différence)		9.99987
Distance méridienne	18	48	50		9 19 9	9.99423
Déclinaison	20	41	7		18 38 18	
Latitude calculée	39	30			20 41 7	
Latitude supposée	39	5			39 19	
Différence	0	25		Somme 71′	40 5	
					0 46	
Equation de la deuxième latitude supposée $\frac{46' \times 60'}{71'}$					0 39	
Latitude du lieu					39 20	

Remarque. Sur la manière d'appliquer l'équation à la latitude calculée par l'une des suppositions, pour déduire la latitude corrigée, je dois aussi renvoyer ici aux pages 60, et 61.

EXEMPLE IV.

Calcul de l'Angle horaire d'un Astre, par sa Hauteur et sa Déclinaison, et la Latitude du lieu.

Hauteur 45° 21′ 54″.	Déclinaison 13° 41′ 36″ N.		Lat. du lieu 23° 20′ N.
Latitude	23° 20′ 0″	C. l. cos.	0.0370551
Déclinaison	13 41 36	C. l. cos.	[0.0125045 184
Distance méridienne au zénith	9 38 24		
Complém. de la haut. a 90°	44 38 6		
Somme	54 16 30		
Demi-somme	27 8 15	L. sin.	[9.6590246 616
Différence	17 29 51	L. sin.	[9.4777409 3408
		Somme	19.1867459
Demi-angle horaire	23 5 2	L. sin. (Demi-somme)	9.5933729
Angle horaire	46 10 4 = 3^h 4′ 40″ 16‴.		

Remarque. Je ne place ici cet exemple que pour en donner un des avantages qu'on peut tirer de disposer les formules de manière à rendre les quantités et leurs variations, ou différences, additives ; en réduisant par ce moyen les opérations à la simple addition totale, et en épargnant la peine d'appliquer séparément les parties proportionelles. Dans le calcul précédent (qui a été fait avec des tables qui donnent les logarithmes de minute en minute), on voit que pour chaque sinus, ou chaque complément arithmétique de cosinus (ou sécante), j'ai pris ce qui convient aux dégrés et aux minutes, et que j'ai écrit dessous les parties proportionelles pour les secondes, afin d'ajouter le tout ensemble.

EXEMPLE V.

Calcul des Équations qu'on doit appliquer à la Distance apparente de la Lune au Soleil, ou à une Étoile, pour avoir la Distance vraie.

Hauteur appar. ☉ 6° 27′ 34″. Hauteur appar. ☾ 54° 11′ 57″. Distance appar. ☉ ☾ 108° 42′ 3″
Correction de la haut. ☉ 7′ 33″. Correction de la haut. ☾ 31′ 42″. Parallaxe horizontale ☾ 55′ 19″

Distance ☉ ☾	-	108° 42′ 3″	C. l. sin.	-	0.0236	-	-	-	-	0.0236
Hauteur ☉	- -	6 27 34	C. l. cos.	-	0.0028					
Hauteur ☾	- -	54 11 57	C. l. cos.	-	- - - -	-	-	-		0.2329
Somme	- - -	169 21 34								
Demi-somme	-	84 40 47	L. cos.	- -	8.9669	-	-	-	-	8.9669
Première différence		78 13 13	L. sin.	-	- - - -	-	-	-	-	9.9908
Deuxième différence		30 28 50	L. sin.	-	9.7053					
			L. constant		0.3010	-	-	-	-	0.3010
Correction haut. ☉		453	L.	- - -	2.6561	C. h. ☾ 1902″	L.	- - -		3.2792
Première équation		45,3	L. (somme)		1.6557	Deux. éq. 622,9	L. (somme)			2.7944

Distance apparente	- - - -				108° 42′ 3″	
Correction haut. ☾	-	— 31′ 42″	Correction haut. ☉	+	0 7 33	
Première équation	-	— 45,3	Deuxième équation	+	0 10 22,9	
					108 59 58,9	
	- - -			—	0 32 27,3	
Distance corrigée des équations principales			-		108 27 31,6	

Remarque. La distance vraie, selon la méthode de M. de Borda, est presque la même (voyez l'exemple dans les Tables de Logarithmes de Callet), mais, cependant, je déduirai les autres corrections, pour montrer la manière de faire ces calculs.

Correct. haut. ☉ 453″					
Demi-prem. éq. 23	L. Prem. éq.	1.6557			
Différence - - 430	L. - - - -	2.6335			
Distance appar. - -	L. cot. -	9.5295			
	L. constant	4.6856			
Troisième équat. 0,0	L. (somme.)	8.5043 (— 10)			
Troisième équat. - -	Demi-L -	4.2521 (— 5)			
Quatrième équat. - -	Demi-L -	0.1056			
Distance appar. - -	C. l. cos. -	0.4940			
	L. constant	0.3010			
Cinquième correct. 1,4	L. (somme)	0.1527			

Correct. haut. ☾ 1902″					
Demi-deux. équat. 311	L. Deux. éq.	2.7944			
Différence - - 1591	L. - -	3.2017			
Distance appar. - - -	L. cot. - -	9.5295			
	L. const. -	4.6856			
Quatrième équat. 1,6	L. (somme)	0.2112			
Distance précédente -	- - - -	108° 27′ 31″,6			
Troisième éq. — 0″,0					
Quatrième éq. — 1,6					
Cinquième éq. + 1,4	- - -	—			0,2
Distance réduite - -	- - - -	108 27 31,4			

Les équations troisième et quatrième seroient positives, si la distance n'excédoit pas 90°, et c'est la seule distinction de cas qu'il faut faire dans le procédé ci-dessus.

EXEMPLE VI.

Calcul des Équations qu'on doit appliquer à la Distance apparente de la Lune au Soleil, ou à une Étoile, pour avoir la Distance vraie.

En se servant des Requisite Tables.

Hauteur apparente ☾	-	49° 57′		Hauteur apparente ✱	-	64° 19′		
Parallaxe horizontale ☾	-	57 8″.		Distance apparente ☾ ✱	-	29 24 46″		
Distance -	29° 24′ 46″	L. sin. -	9.6912	- - -	-			9.6912
Hauteur ☾ -	49 57 0	L. cos. - -	- - -	- - - -	-			9.8085
Hauteur ✱ - -	64 19 0	L. cos. - -	9.6369					
Somme - -	143 40 46							
Demi-somme -	71 50 23	L. sec. - -	0.5061	- - -	-			0.5061
Première différence	21 53 23	L. cosec. -	0.4286					
Deuxième différence	7 31 23	L. cosec. - -	- - -	- - - -	-			0.8833
Correct. de la haut. ✱	0 0 27	L. p. - - -	2.6021	Correct. haut. ☾	35′ 58″	L. p		0.6994
		L. constant -	9.6990	- - -	-			9.6990
Première équation -	0 0 29	L. p. (somme)	2.5039	Deux. éq.	9′ 17″	L. p. (som.)		1.2875
Correct. haut. ☾ -	0 35 58							
Demi-deux. équation	0 4 38	L. p. deux. éq.	1.2875	Distance appar. - -			29° 24′ 46″	
Différence -	0 31 20	L. p. -	0.7592	Correct. de la haut. ✱	+		0 0 27	
		L. tan. distance appar.	9.7512	Deuxième équation	+		0 9 17	
		L. constant - - -	1.2810					
		L. p. (somme) - - -	3.0789	Troisième équation	+		0 0 9	
		Correct. de la haut. ☾	— 35′ 58″				29 34 39	
		Première équation -	— 29	- - - - -	—		0 36 27	
				Distance réduite - -			28 58 12	

Remarque. Quand la distance excéde 90°, la troisième équation devient négative.

Je ne déduirai pas les autres équations ; car le dégré d'approximation du calcul qui précéde est celui que la plupart des Navigateurs estimeront suffisant pour la pratique.

Le même exemple calculé par la méthode de Mr. WITCHELL (voyez les *Requisite Tables*) donne 28° 58′ 11″, pour la distance réduite.

DE MENDOZA Y RIOS.

Londres :
4 Novembre, 1796.

ADDITION.

Contenant une Méthode pour réduire les Distances lunaires. Par Mr. H. Cavendish, *Membre de la Société Royale*, &c.

Mr. CAVENDISH m'ayant fait l'honneur de me communiquer la méthode qu'il a trouvé pour réduire les distances lunaires, je profite de la permission de ce savant, pour la faire connoître au public, en plaçant ici un extrait de ce qu'il m'a écrit à ce sujet, dans les propres mots de l'auteur.

Extract of a Letter from Henry Cavendish, *Esq. to Mr.* Mendoza y Rios, *January*, 1795.

"The methods in which the whole distance of the moon and star is computed, particularly yours, require fewer operations than those in which the difference of the true and apparent places is found; but yet, as in the former methods, it is necessary either to take proportional parts, or to use very voluminous tables; I am much inclined to prefer the latter. This induced me to try whether a convenient method of the latter kind might not be deduced from the fundamental proposition used in your paper, and I have obtained the following, which has the advantage of requiring only short tables, and wanting only one proportional part to be taken, and I think seems shorter than any of the kind I have met with.

"Let b and H be the apparent and true altitude of the star;

l and L the apparent and true altitude of the moon, g and G the apparent and true distance of the moon and star. Let the sine and cosine of $g = d$ and δ, the sine and cosine of $l = a$ and α, the sine and cosine of $b = b$ and β; and the sine of the actual and mean horizontal parallax $= p$ and π; and let the sine of $L = a - m + p e$, and its cosine $= \alpha(1 + \mu - p\varepsilon)$ and let the sine of $H = b - n$, and its cosine $= \beta(1 + \nu)$.

" Then the cosine of $G = \delta(1+\mu-p\varepsilon)(1+\nu)+(a-m+pe)(b-n)-ab(1+\mu-p\varepsilon)(1+\nu)$, which equals $\delta+\delta\mu+\delta\nu-\delta p\varepsilon+\delta\mu\nu-\delta p\varepsilon\nu+ab-bm+bpe-an+nm-npe-ab-ab\mu+abp\varepsilon-ab\nu-ab\mu\nu+ab\nu p\varepsilon = \delta+\delta\mu+\delta\nu-\delta p\varepsilon-bm-ba\mu+bpe+bap\varepsilon-an-ab\nu+nm-npe-ab\mu\nu+ab\nu p\varepsilon+\delta\mu\nu-\delta\pi\varepsilon\nu$.

" To make use of this rule, it must be considered that the quantity $\delta\mu\nu - \delta p\varepsilon\nu$ is so small that it may safely be disregarded; but $nm - npe - ab\mu\nu + ab\nu p\varepsilon$, if the altitudes are not more than 5°, may amount to about 12″, and therefore ought not to be neglected. The quantity $e + a\varepsilon$ also differs very little from one, but is not quite equal to it. Let therefore a table be made under a double argument, namely, the altitudes of the moon and star, giving the value of . . . . $nm - n\pi e - ab\mu\nu + ab\nu\pi\varepsilon + b\pi e + ba\pi\varepsilon - b\pi$, answering to different values of these altitudes, which call A. Let a second table be made under a double argument, namely, the altitude of the star and the apparent distance of the moon and star, giving the value of $\delta\nu$, which call D. Let a third table be made with the observed altitude for argument, giving the logarithm of $am + a^3\mu$; and let this quantity, answering to the moon's altitude, be called M, and that answering to the

star's altitude, N; observing that the same table will do for the moon and star; but a fourth table should be made for the sun, so as to include its parallax; and, lastly, let a fifth table be made, with the moon's altitude for argument, giving the logarithm of $\frac{s}{a} - \frac{\mu}{\pi a}$, which call C. Then will $\cos. G = \delta - \delta a p C - \frac{b M}{a} - \frac{a N}{b} + b p + D - A$.

"It must be observed that $\delta a p C = \delta p \varepsilon - \frac{\delta \mu p}{\pi}$, whereas it ought to equal $\delta p \varepsilon - \delta \mu$; but μ cannot exceed $57''$, and the horizontal parallax cannot differ from the mean by more than $\frac{1}{15}$ part of the whole; so that the error arising from thence cannot exceed $3''$ or $4''$. This small error however may be diminished by giving the quantity C for more than one horizontal parallax."

Addition to the foregoing Letter.

"I have procured tables of the above-mentioned kind to be computed, which are intended to be inserted in a work now printing by Mr. Mendoza y Rios. Allowance is made in them for the alteration of the refractive power of the atmosphere, which is done by two new tables, one giving the correction of the logarithms M and N, and the other the sum of the corrections of $\delta \mu$ and $\delta \nu$. Now it must be observed, that the quantities μ and ν vary only from $57''$ to $51''$; and therefore the corrections of $\delta \mu$ and $\delta \nu$, may, without any material error, be considered as the same at all altitudes; and therefore the sum of the corrections may be comprehended in a table, under a double argument, namely, the refractive power of the atmosphere and the apparent distance.

"In order to avoid as much as possible the inconvenience arising from using negative quantities, or giving different cases, the table D is continued to 125° of apparent distance, and the numbers in the table A are increased by 0,0003, so as to make them always positive; and to compensate this, the numbers in D are increased by 0,0002, and those in the correction of $\partial\mu + \partial\nu$ by 0,0001. It was found proper also to give the table C for four different values of horizontal parallax.

"The above tables are short, and do not require proportional parts to be taken. The only part of the work in which this is wanted, is in finding the angle answering to the natural cosine of the true distance. In finding the natural cosine of the apparent distance this is avoided, by neglecting the odd seconds in working the problem, and adding them to the result."

IV. *On the Nature of the Diamond.* *By* Smithson Tennant, *Esq. F. R. S.*

Read December 15, 1796.

Sir Isaac Newton having observed that inflammable bodies had a greater refraction, in proportion to their density, than other bodies, and that the diamond resembled them in this property, was induced to conjecture that the diamond itself was of an inflammable nature. The inflammable substances which he employed were camphire, oil of turpentine, oil of olives, and amber; these he called "fat, sulphureous, unctuous "bodies;" and using the same expression respecting the diamond, he says, it is probably "an unctuous body coagulated." This remarkable conjecture of Sir Isaac Newton has been since confirmed by repeated experiments. It was found that, though the diamond was capable of resisting the effects of a violent heat when the air was carefully excluded, yet that on being exposed to the action of heat and air, it might be entirely consumed. But as the sole object of these experiments was to ascertain the inflammable nature of the diamond, no attention was paid to the products afforded by its combustion; and it still therefore remained to be determined whether the diamond was a distinct substance, or one of the known inflammable bodies. Nor was any attempt made to decide this question till M. Lavoisier, in 1772, undertook a series of

experiments for this purpose. He exposed the diamond to the heat produced by a large lens, and was thus enabled to burn it in close glass vessels. He observed that the air in which the inflammation had taken place had become partly soluble in water, and precipitated from lime-water a white powder which appeared to be chalk, being soluble in acids with effervescence. As M. LAVOISIER seems to have had little doubt that this precipitation was occasioned by the production of fixed air, similar to that which is afforded by calcareous substances, he might, as we know at present, have inferred that the diamond contained charcoal; but the relation between that substance and fixed air, was then too imperfectly understood to justify this conclusion. Though he observed the resemblance of charcoal to the diamond, yet he thought that nothing more could be reasonably deduced from their analogy, than that each of those substances belonged to the class of inflammable bodies.

As the nature of the diamond is so extremely singular, it seemed deserving of further examination; and it will appear from the following experiments, that it consists entirely of charcoal, differing from the usual state of that substance only by its crystallized form. From the extreme hardness of the diamond, a stronger degree of heat is required to inflame it, when exposed merely to air, than can easily be applied in close vessels, except by means of a strong burning lens; but with nitre its combustion may be effected in a moderate heat. To expose it to the action of heated nitre free from extraneous matters, I procured a tube of gold, which by having one end closed might serve the purpose of a retort, a glass tube being adapted to the open end for collecting the air produced. To be certain that the gold vessel was perfectly closed,

and that it did not contain any unperceived impurities which could occasion the production of fixed air, some nitre was heated in it till it had become alkaline, and afterwards dissolved out by water; but the solution was perfectly free from fixed air, as it did not affect the transparency of lime-water. When the diamond was destroyed in the gold vessel by nitre, the substance which remained precipitated lime from lime-water, and with acids afforded nitrous and fixed air; and it appeared solely to consist of nitre partly decomposed, and of aerated alkali.

In order to estimate the quantity of fixed air which might be obtained from a given weight of diamonds, two grains and a half of small diamonds were weighed with great accuracy, and being put into the tube with a quarter of an ounce of nitre, were kept in a strong red heat for about an hour and a half. The heat being gradually increased, the nitre was in some degree rendered alkaline before the diamond began to be inflamed, by which means almost all the fixed air was retained by the alkali of the nitre. The air which came over was produced by the decomposition of the nitre, and contained so little fixed air as to occasion only a very slight precipitation from lime-water. After the tube had grown cold, the alkaline matter contained in it was dissolved in water, and the whole of the diamonds were found to have been destroyed. As an acid would disengage nitrous air from this solution as well as the fixed air, the quantity of the latter could not in that manner be accurately determined. To obviate this inconvenience, the fixed air was made to unite with calcareous earth, by pouring into the alkaline solution a sufficient quantity of a saturated solution of marble in marine acid. The vessel which contained

them being closed, was left undisturbed till the precipitate had fallen to the bottom, the solution having been previously heated that it might subside more perfectly. The clear liquor being found, by means of lime-water, to be quite free from fixed air, was carefully poured off from the calcareous precipitate.* The vessel which was used on this occasion was a glass globe, having a tube annexed to it, that the quantity of the fixed air might be more accurately measured. After as much quicksilver had been poured into the glass globe containing the calcareous precipitate as was necessary to fill it, it was inverted in a vessel of the same fluid. Some marine acid being then made to pass up into it, the fixed air was expelled from the calcareous earth; and in this experiment, in which two grains and a half of diamonds had been employed, occupied the space of a little more than 10.1 ounces of water.

The temperature of the room when the air was measured, was at 55°, and the barometer stood at about 29.8 inches.

From another experiment made in a similar manner with one grain and a half of diamonds, the air which was obtained occupied the space of 6.18 ounces of water, according to which proportion the bulk of the fixed air from two grains and a half would have been equal to 10.3 ounces.

The quantity of fixed air which was thus produced by the diamond, does not differ much from that which, according to M. LAVOISIER, might be obtained from an equal weight of charcoal. In the Memoirs of the French Academy of Sciences for

* If much water had remained, a considerable portion of the fixed air would have been absorbed by it. But by the same method as that described above, I observed, that as much fixed air might be obtained from a solution of mineral alkali, as by adding an acid to an equal quantity of the same kind of alkali.

the year 1781, he has related the various experiments which he made to ascertain the proportion of charcoal and oxygen in fixed air. From those which he considered as most accurate, he concluded that 100 parts of fixed air contain nearly 28 parts of charcoal and 72 of oxygen. He estimates the weight of a cubic inch of fixed air under the pressure and in the temperature abovementioned, to be .695 parts of a grain. If we reduce the French weights and measures to English, and then compute how much fixed air, according to this proportion, two grains and a half of charcoal would produce, we shall find that it ought to occupy very nearly the bulk of 10 ounces of water.

M. Lavoisier seems to have thought that the aerial fluid produced by the combustion of the diamond was not so soluble in water as that procured from calcareous substances. From its resemblance, however, in various properties, hardly any doubt could remain that it consisted of the same ingredients; and I found, upon combining it with lime, and exposing it to heat with phosphorus, that it afforded charcoal in the same manner as any other calcareous substance.

V. *A Supplement to the Measures of Trees, printed in the Philosophical Transactions for* 1759. *By* Robert Marsham, *Esq. F. R. S.*

Read December 22, 1796.

THESE measures were all taken by myself, except the second, of the ash in Scotland; and that I believe is fair. As that is the largest ash, and as thriving as any I had seen, I was desirous to procure a second measure of it. The measures (where there was no impediment) were taken at five feet from the earth, as the easiest height to run the line even, and a fair height for the bulk of the body. For most trees (at least oaks and chesnuts) are frequently found to be one-third more in circumference at one foot than at five. Where I have measures of more than one tree of the same kind, I give the largest and a smaller, to show the different proportion of the increase of their different sizes: and as trees standing single generally increase more than those in groves, I mark them with an S. and a G. as the difference is more than would be expected by those that think little of trees.

In 1719 I had about two acres sowed with acorns, and from 1729 to 1770 I planted oaks from this grove, always leaving the best plants standing for the future grove: but most of the transplanted trees are already larger than those that were not removed; the largest of which is now (1795) but five feet

6 inches 8 tenths in circumference; and the largest transplanted tree (which was planted in 1735) is 8 ft. 8 in. 7 tenths, viz. near 38 inches gained by transplanting in 60 years. And in beeches from seed, in 1733, the largest is now (1795) but 6 feet 9 inches; and the largest transplanted beech is 7 feet 5 inches 1 tenth, viz. 8 inches larger, although the transplanted beech is eight years younger than that from the seed. This proves that it is better to plant a grove, than to raise one from the seed. The expence of planting is inconsiderable, and the planted trees are full as good and handsome; and many years are saved, beside the extra growth of planted trees. But this extra growth will not prove near so great in groves as in single trees. The first grove I planted from these acorns of 1719, was in 1731. In 1732 I made another grove from them; and in 1735 I planted a third grove from them; and in 1753 the last considerable number of plants were taken from the grove, and these are very good trees: so 34 years may be saved. But I would by no means advise the planting trees so large, as the trouble and expence will be too much, unless where a shelter or screen is wanted.

Whether a grove is to be raised from seeds, or planted, it is advisable to shelter it round; if from the seed, with such sorts as will grow quicker; and if by planting, with larger and taller trees. The soil in Norfolk is unfavourable to elms; therefore in planting I will venture to recommend hornbeams, as they may be planted large trees. I planted some hornbeams (rather large) in 1757, and disliking their situation, in 1792 I removed them when they were about three feet in circumference, and did not lose one tree; and they made shoots of

near half a yard that year; but I ought to say I cut off their heads.

Before I quit this subject, I will presume to recommend, if young oaks are unthriving, there is reason to hope they may be helped by cutting them down to a foot or six inches: for in 1750 I planted some oaks from my grove of 1719 into a poorer soil, and although they lived, they were sickly; so in 1761 I cut most of them down to one foot, and then by cutting off the side shoots, in three or four years led them into a single stem, and most of them are now thriving and handsome trees; and you can hardly see where they were cut off, and some are four feet round; and I have used the same method with unhealthy chesnuts, beech, hornbeam, and wych elm, and with the same success.

R. MARSHAM.

Stratton, May 29. 1796.

The aggregate Increase in Circumference of different Trees, divided into tenths of Inches of their annual Growth.

	Dates.	Feet.	Inches.	tenths of In.	Feet.	Inches.	tenths of In.	Years.	tenths of In.
S. Oak, in the Holt Forest, by the Lodge -	1759	34	0	2+					
	1778	34	0	7+					1/2
S. Oak, in Stratton, planted in 1580, at 4 feet	1760	15	2	9					
	1781	16	5	8	1	2	9	21	- +7
S. Oak, planted by me, in 1720 - -	1742	2	11	2					
	1781	8	2	6	5	3	4	39	- 16½
S. Oak, acorn in 1719, and transplanted 1735	1756	3	6	0					
	1781	7	2	2	3	8	2	25	about 17½
S. Wych elm, in Stratton Hollow, at 4 feet	1760	29	5	6					
	1780	29	10	0	0	4	4	20	- 2½
S. Wych elm, by Bradly church, Suffolk -	1744	25	5	4					
	1765	26	0	0	0	7	2	11	- 6½
G. Wych elm, in Stratton - - - -	1787	3	9	6					
	1795	4	6	0	0	9	9	8	- +11
S. Ash, in Benel ch. yd. N. of Dunbarton, Scotland	1768	16	9	6					
	1783	18	0	0	1	3	0	15	- 10
S. Ash, in Stratton, planted after 1647 -	1742	9	10	5					
	1782	12	11	2	3	0	7	40	- +9
S. Ash, planted in 1725, in very poor land -	1769	5	5	0					
	1781	6	6	1	1	1	1	12	near 11
S. Chesnut, in Christ Church Park, by Ipswich	1747	15	8	3					
	1765	16	11	2	1	3	7	16	- +9
S. Chesnut, in Hevingham, Norfolk, planted 1610	1742	12	7	0					
	1781	14	11	3	2	4	3	39	near 7½
S. Beech, in Christ Church Park, by Ipswich	1755	15	7	5					
	1763	15	10	0	0	3	1	8	near 4
S. Beech, in Stratton, seed 1741, washed and dried	1778	3	7	4					
	1781	4	4	4	0	9	0	3	- 30
G. Beech, same age - - - - -	1785	3	10	5					
	1795	5	1	5	1	3	0	10	- 15
S. Plane, in Shottisham, Norfolk - -	1755	3	10	3					
	1774	7	9	2	3	10	9	19	- +24½
S. Poplar, black, set in my father's time -	1756	11	5	0					
	1768	12	2	4	0	9	4	12	near 8
S. Poplar, black, in Horstead, Norfolk -	1750	6	1	0					
	1754	7	4	0	1	3	0	4	- 37½
S. Poplar, white Abele - - - - -	1760	0	7	0					
	1781	4	3	5	3	8	5	21	- +21

	Date	Feet	Inches	10ths of In.	Feet	Inches	10ths of In.	Years	10ths of In.
S. Willow	1756	5	0	0					
	1765	6	4	2	1	4	2	9	- 18
G. Alder, in sandy soil	1759	2	0	4					
	1776	3	4	7	1	4	3	17	- +9½
S. Asp	1772	2	8	7					
	1781	4	2	0	1	5	3	9	- +19
G. Mountain ash	1759	2	2	7					
	1781	4	2	4	1	11	7	22	- +10½
G. Birch	1759	2	10	4					
	1768	3	6	2	0	7	8	9	- 8½
G. Horsechesnut	1758	1	4	4					
	1779	3	0	2	1	7	8	21	near 9½
G. Lime, in sandy soil	1727	3	2	5					
	1787	3	9	0	0	6	5	6	near 11
G. Cedar, one foot high in 1748	1777	3	1	0					
	1795	6	1	5	2	11	9	18	almost 20
G. Silver fir, planted in 1746	1758	1	6	5					
	1781	4	10	0	3	4	1	23	near 18
G. Scotch fir, planted in 1735	1756	4	1	5					
	1781	6	8	0	2	6	5	25	- 13½
G. Spruce fir, planted 1735	1756	3	4	9					
	1781	5	2	0	1	9	1	25	near 8½
S. Weymouth pine, planted in 1747	1756	1	4	1					
	1781	4	8	5	3	4	4	25	- +16
G. Pinaster, planted in 1738	1756	4	0	7					
	1702	4	11	5	0	10	8	6	- 16
G. Larch, planted in 1749	1758	1	5	2					
	1781	4	2	5	2	9	3	23	near 14½
S. Holly, from seed, by me, and transplanted	1749	1	10	4					
	1781	3	9	1	1	10	7	32	- +7
S. Hawthorn, by Hethel church, Norfolk, at 4 ft.	1755	9	1	0					
	1781	9	8	5	0	7	5	26	near 3

VI. *On the periodical Changes of Brightness of two fixed Stars.* By Edward Pigott, *Esq.* *Communicated by* Sir Henry C. Englefield, *Bart. F. R. S.*

Read January 12, 1797.

Bath, August, 1796.

ALTHOUGH those far distant suns, the fixed stars, have baffled all investigation with regard to our knowledge of their distance, magnitudes, and attractions; we have, nevertheless, by determining their periodical changes of light, established a strong affinity between them and our sun; and among such an inconceivable number, we may expect to find some with periods of rotation much longer and shorter than those we are already acquainted with, and with changes perhaps even sufficiently rapid to afford a ready means for determining accurately differences of terrestrial longitudes. This would be a most satisfactory, useful, and profitable discovery, and may be the lot of those who have but a slight knowledge of astronomy, provided that with great exactness, and a good memory, a constant look out be given. The discoveries which at present I have the honour of laying before the Society, are the periodical changes of brightness of two stars, one in *Sobieski's Shield*, the other in the *Northern Crown*.

The constellation of *Sobieski's Shield* consists of a very few stars, and was formed by HEVELIUS, in honour of a *king of Poland*; the variable star that now appears in it was, doubtless, not noticed by him, as he has set down stars near it, which are by times much less conspicuous. It has nearly the same right ascension as the star *l*, and is about one degree more

south: this, for the present, suffices to point out its place; for as I wish to proceed immediately to the results, I shall, for greater perspicuity, collect at the end of this account, a more exact determination of its right ascension and declination, as also a plan of the stars situated near it.

When at its full and least brightness, it attains in different periods, different degrees of brightness: I have never yet seen it of a greater magnitude than of the 5th, nor when at its least, less than the 7.8th. It completes all its changes in about 63 days, being 14 = at its full brightness, without any perceptible change; 9 = at its least, also without any perceptible change; 18 = days decreasing from the middle of its full brightness to the middle of its least; and 35 = increasing from the middle of its least brightness to the middle of its full. These results being deduced from only the few observations I have made, cannot, of course, be very accurate, but may easily and soon be corrected by comparing any future observation with those communicated in this paper; not relying much on the estimated magnitudes, but principally on its comparative brightness with the stars there mentioned and marked in the plan, the magnitudes of which, by a mean of several observations, I have settled thus:

Magnitudes.	
	λ 3
	i 4
	m 4
	l 4.5
	o 4.5
	k 5
	n 5
	b 5.6
	g 5.6
* above	*f* 6
	p 6.7
	neb. 6.7
	r 7
	t 8

The nine first letters are according to FLAMSTED, the others as affixed by me.

Extract from my Journal of the Observations on the Variable in *Sobieski's Shield*; made at Bath.

Date.	Magnit.	
1795. Sept. 25	5	brighter than *k*, and less than *l*; it has lately been increasing.
Oct. between 1 & 9	5	ditto ditto.
26	5	rather less than *k*; much brighter than *P*.
30	6	much less than *k*, and rather brighter than *P*.
Novemb. 6 & 7	6	*much* less than *k*, and *rather* brighter than *P*.
14	5	almost equal to *k*, and much brighter than *P*.
27	5	I think *rather* less than *k*.
Decemb. 24	5	I could not determine which was brightest, the variable, or *k*.
1796. Feb. 12 & 13	6	considerably less than *k*, and *rather* brighter than *P*.
March 4	7	much less than *P*.
22	6	rather brighter than *P*; considerably less than *k*.
April 7, 17, 19	5	considerably brighter than *P*, and *rather* less than *k*.
30	7	less than *P*; brighter than *P*.
May 4, 10, 12, 13	7.8	much less than *P*, and rather less than *P*. The observation of the 12th seems to express most decidedly its being less than *P*.
16	7	equal, or *rather* brighter than *P*; much less than *P*.
19	6	*rather* brighter than *P*.
24	6.5	brighter than *P*; much less than *k*.
31	5.6	much brighter than *P*; rather less than *k*.
June 2	5	not quite so bright as *k*.
9, 10	5	rather brighter than *k*; considerably less than *l*.
14	5	brighter than *k*; *much* less than *l*.
15, 20, 24	5	ditto, ditto, ditto.
25	5	rather brighter than *k*.
29	3	if any difference, brighter than *k*; decreased.
July 7, 8	5	equal to *k*.
16	5	*rather* less than *k*; considerably brighter than *P*; near its full.
19	5.6	less than *k*; much brighter than *P*. ditto.
26, 27	5	*rather* less than *k*; considerably brighter than *P*.
August 4, 7	5.6	less than *k*; much brighter than *P*.
12, 15	5.6	ditto ditto; more near them.
19, 21, 22	5.6	between the brightness of *k* and *P*.
27	5.6	ditto ditto, or less bright.
29	6.5	much less than *k*; rather brighter than *P*.
Sept. 4, 5	6	considerably less than *k*; rather brighter than *P*.
7	6	ditto ditto ditto; I think it rather increased.
8	6.5	less than *k*; brighter than *P*.
16	5	rather less than *k*; considerably brighter than *P*.

From these observations the periodical changes were deduced as follows:

The length of a single period being first settled of 67 days, from a succession of observations between March and May, and of 69 between April and June, we may proceed to obtain a greater exactness from distant dates, thus:

				DAYS.
		Middle of its greatest brightness.		
1795. Oct.	1st.	Interval of four periods, making the		
1796. June	18	length of a single one	- -	$65\frac{1}{4}$
1795. Oct.	1	Interval of three periods, making the		
1796. April	10	length of a single one	- - -	64
		Middle of its least brightness.		
1795. Nov.	6	Interval of three periods, making the		
1796. May	10	length of a single one	- -	62
1795. Nov.	6	Interval of two periods, making the		
1796. March	4	length of a single one	- -	$59\frac{1}{2}$
		A single period, on a mean	- -	$62\frac{1}{4}$

Had it been requisite to have given any preference to one of these four results, I should have chosen the third; not only on account of the exactness of the observations themselves, but particularly because the changes when near its least brightness are quicker; however, they all agree more satisfactorily than I think could be expected; still it must be remembered, that the mean period here determined is merely for this set of observations, it being yet unknown what kind of irregularities it is liable to; for while I am now writing, in the month of August, its changes seem different from those of the four preceding periods; and how these perturbations will terminate,

cannot be settled in the present account, as I mean here to conclude it; but will add in the Journal, observations of as late a date as possible.

The mean right ascensions of the stars here given, were deduced from observations made in the meridian with a small transit instrument, and are, I believe, accurate. The declinations are not settled with greater precision than to two or three minutes; and although quite sufficient to prevent any mistake, I have, for the satisfaction of those who wish to make further observations on them, drawn up the annexed plan, in which all the stars they were compared to, can easily be found; no greater exactness is intended. (See Tab. II.)

	Mean right ascension.			Declination		
	In Time.			in Degrees, &c.		
	h	′	″	°	′	″
Computed for June 25th, 1796.						
The little star τ in my plan, in Sobieski's shield	18	36	16,7:	279	4	10:	6	7½	S
The variable in Sobieski's shield - - -	18	36	38,5	279	9	37	5	56	S
Computed for June 1st, 1796.									
The little star ο of my plan in the Northern crown	15	39	20,6	234	50	9	29	8	N
The variable in the Northern crown - -	15	40	11,4	235	2	51	28	49½	N

THE other Variable that I have discovered is, as already mentioned, in the *Northern Crown*. Its right ascension and declination have just been given, as likewise the plan of the stars near it. This star, although not in FLAMSTEAD's catalogue, is marked on BAYER's maps of the 6th magnitude. Several years ago, in 1783, 1784, and 1785, I suspected it to be changeable, which induced me to make the memorandums here copied in the Journal, since which time I have often seen it, but not perceiving any alteration, the dates were neglected until the spring of 1795; I then had the satisfaction of finding my suspicions confirmed, it being invisible; but on the 20th of June, it appeared of the 9.10th magnitude, and went through

its various changes as follows: in six weeks it had increased to its full brightness, the middle time of which was August 11th, 1795. At its full brightness it was of the 6.7th magnitude, and remained the same without any perceptible alteration for about three weeks: it then was three weeks and a half in decreasing to the 9.10th magnitude, and disappeared a few days after. Having reappeared in the following April, 1796, it was on the 7th of May again of the 9.10th magnitude, and increasing nearly in a similar manner as on the 20th of June the preceding year; which completes all its changes, and gives a period of ten months and a half.

Very remarkable and perplexing it was, that just after I had made out the periods of these two variable stars, their changes should appear different from those before observed; the particulars concerning that in *Sobieski's Shield* have been noticed: as for this in the *Northern Crown*, it shews at present (being the computed time of its full brightness), great unsteadiness, more so, I think, than any of the variables whose periods have been settled with certainty; for having increased as before, with tolerable regularity, till it attained the 7.8th magnitude, it then kept wavering between those magnitudes, and is still so at the present time (August) that I am closing my account of it. I nevertheless hope to add a few more remarks in the Journal, as I have done for the other variable. Future observations will determine how far the period of ten months and a half is rightly settled. I am greatly inclined to think it the true one, as the star went through all its changes progressively and steadily. Many of the variables are occasionally liable to unexpected changes, particularly at the attainment of their full brightness in different periods; such perturbed periods may

perhaps be found to return after a certain number of more regular ones; but to ascertain this, requires probably a long series of observations. The magnitude of the stars in the *Northern Crown*, marked on my plan, and to some of which the variable was compared, are here accurately fixed by a mean of many observations. (See Tab. II.)

Magnitudes.		
α	2.3	All these characters are according to BAYER, except the four last, which I have added.
β, θ	4.3	
γ		
ε	4	
δ, ι	5	
ζ	6	
κ	6.7	
ω	7	
ο	8.9	
P	9	
χ	10	

I have in this paper followed, as much as possible, the same method and deductions as in my others, which the Society have done me the honour of publishing.* The subject of them all being very similar, it was difficult to avoid sometimes repeating the same remarks, which, if omitted, might perhaps occasion some uncertainty, and perplex those who do not recollect or have not read the former papers. I shall now conclude with my observations on the variable in the Crown.

* See *Phil. Trans.* Vol. 75, and 76, &c.

Extracts from my Journal, of the Observations on the Variable in the *Northern Crown;* made at Bath.

Dates.			Magnit.	
1783.	July	27	7.8	seen with difficulty with an opera-glass.
		30	7	much brighter.
		31	7	though the air was hazy, I could see it *with* D°.
	August	8	7	saw it distinctly—opera-glass.
				as, in these four observations, it was not compared to any star, they are less to be relied on.
1784.	July	11	6.7	thought it considerably brighter than last year.
				rather less than τ, but evidently brighter than w.
		14	6.7	not so bright as ρ, equal to τ, and brighter than w.
1785.	May	20	7	it is marked less than τ, and brighter than the 7.8th magnitude
1795.	May	28	.	not visible with an opera-glass.
	June	20	9.10	evidently less than o; rather less than P; rather brighter than x.
		23	9	equal to, or brighter than P.
		29	8.9	evidently brighter than P; nearly equal to o.
	July	6		
		7	7	evidently brighter than o; nearly equal to w.
		13		
		24		
		25	6.7	certainly brighter than w, and rather less than τ ρ.
		31		
	August	2		
		6		
		11	6.7	nearly equal to τ; no perceptible alteration during these dates.
		17		
		21		
		28	7.6	less than τ; moon nearly full.
	Sept.	4		
		6	7	evidently less than τ; if *any difference* brighter than w.
		13	8.7	evidently less than w; if any difference brighter than o.
		15	9	less than o, and equal to P.
		16	9.10	equal to, or less than P; brighter than x.
		20		not visible with an excellent night-glass; therefore less than the
		22	.	11th magnitude; a remarkably rapid disappearance; air clear.
	Nov.	1	.	
	Dec.	12	.	
1796.	Jan.	11	.	not visible with an opera-glass, with which I can, when the air
	Feb.	12	.	is very clear, see the star o of my plan.
	March	27	.	
		28	.	not visible with the night-glass; therefore not of the 11th magnit.
	April	14		
		17	10	visible with night-glass; less than x.
		25		
	May	1	9.10	brighter than x; rather less than P.
		10		
		12	9	less than o, and equal to, or rather brighter than P.
		19	8.9	equal to, or *rather* brighter than o. ☽ near full.

Continuation of the Observations on the variable Star in the *Northern Crown*. Bath.

Dates.		Magnit.	
1796. May	24	8	rather brighter than *o*.
	31		
June	9	7.8	brighter than *o*; less than *w*.
	10		
		7.8	between the 10th and 24th I often tried to see it with an opera-glass, but owing to the moon and twilight, I could not, though the *w* was by times perceptible, therefore it could not be brighter than the 7.8th magnitude.
	24		
	25		
	29	8	*rather* brighter than *o*; considerably less than *w*.
July	7		
	8		
	23		
	25		
	26		during these dates it has in general been set down much brighter than *o*, and rather less than *w*, though sometimes more decidedly less than *w*; but these very small differences are ever difficult to ascertain, owing to the disposition of the eye, atmosphere, and various lights.
	27		
	30	7.8	
Aug.	4		
	7		
	12		
	15		
	19		
	21	7	equal to *w*; no moonlight.
	22		
	27	7	equal to, or rather less than *w*.
Sept.	4	7	equal to, if not brighter than *w*.
	5		
	8	7	equal to, if not less than *w*.

VII. *Experiments and Observations, made with the View of ascertaining the Nature of the Gaz produced by passing Electric Discharges through Water.* *By* George Pearson, *M. D. F. R. S.*

Read February 2, 1797.

§ 1.

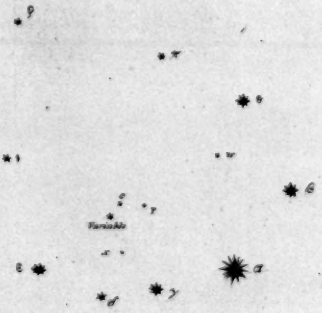
In the *Journal de Physique* for the month of November, 1789, were published the very curious and interesting experiments of Messrs. Paets van Troostwyk and Deiman; which were made with the assistance of Mr. Cuthbertson; on the apparent decomposition of water by electric discharges.

The apparatus employed was a tube 12 inches in length, and its bore was $\frac{1}{4}$ of an inch in diameter, English measure; which was hermetically sealed at one end, but before it was sealed, $1\frac{1}{2}$ inch of gold or platina wire was introduced within the tube, and fixed into the closed end by melting the glass around the extremity of the wire. Another wire of platina, or of gold with platina wire at its extremity, immersed in quicksilver, was introduced at the open end of the tube, which extended to within $\frac{5}{8}$ of an inch of the upper wire, which, as was just said, was fixed into the sealed extremity.

The tube was filled with distilled water, which had been freed from air by means of Cuthbertson's last improved air pump, of the greatest rarefying power. As the open end of the tube was immersed in quicksilver, a little common air was

Sobieski's Shield & part of Antinous.

The Northern Crown.



let up into the convex part of the curved end of the tube, with the view of preventing fracture from the electrical discharge.

The wire which passed through the sealed extremity was set in contact with a brass insulated ball; and this insulated ball was placed at a little distance from the prime conductor of the electrical machine. The wire of the lower or open extremity, immersed in quicksilver, communicated by a wire or chain with the exterior coated surface of a Leyden jar, which contained about a square foot of coating; and the ball of the jar was in contact with the prime conductor.

The electrical machine consisted of two plates of 31 inches in diameter, and was similar to that of TEYLER. It had the power of causing the jar to discharge itself 25 times in 15 revolutions. When the brass ball and that of the prime conductor were in contact, no air or gaz was disengaged from the water by the electrical discharges; but on gradually increasing their distance from one another, the position was found in which gaz was disengaged; and which ascended immediately to the top of the tube. By continuing the discharges, gaz was discharged till it reached to nearly the lower extremity of the upper wire, and then a discharge occasioned the whole of the gaz to disappear, a small portion excepted, and its place was consequently supplied by water.

From my own experience I should venture to affirm, that a more particular and more accurate account than that published is requisite, to enable the student, or even the proficient, to institute the above experiment with success. Hence, during the six or seven years which have elapsed since its publication, no confirmation has been published, except the experiment repeated by Mr. CUTHBERTSON for my satisfaction, as related in

my work on the Chemical Nomenclature; although I have heard of many persons, and some of them experienced electricians and chemists, who have made the attempt. But by labouring with Mr. CUTHBERTSON, since he came to reside in London, I have learned the circumstances on which the success of the experiment depends; and I have received from him effectual aid in continuing a process, with the objects I had in view, the tediousness and even difficulties of which can only be conceived by those who have been engaged in the same pursuit.

In the course of my experiments on this subject, Mr. CUTHBERTSON invented a new method of disengaging gaz from water, by means of the electrical discharges, namely, by means of *uninterrupted* or *complete discharges;* whereas the method of Mr. VAN TROOSTWYK was by *interrupted discharges.* The *rationale* of the process according to these two methods, I apprehend, cannot be understood without an explanation; for I find books on electricity do not contain the necessary information.

In the experiment of Mr. VAN TROOSTWYK, it must be considered, that if in place of water the tubes be filled with air, the whole of the charge of the Leyden jar will pass, at each explosion, from the upper to the under wire, and no interruption in the discharge will happen; but if they are filled with water, then an *interrupted discharge* may be caused: by which is meant, that a part of the charge only passes at each explosion through the water from wire to wire, and with much diminished velocity. The residuary electricity in the Leyden jar is nearly one half, as may be accurately demonstrated. The reason of these differences must be assigned from the difference in point of density, elasticity, and conducting power, of the medium of

water and of air. It must be added, that although water in large quantity is a good conductor, and air is not, yet water being here in very small quantity it proves a bad conductor; as is the case with the very best conductors. A cubic foot of water is only just capable of receiving, or letting pass through it, a full discharge from a jar of one foot of coated surface; and the quantity of water employed in this experiment not being $\frac{1}{1000000}$ part of a cubic foot it is a very imperfect conductor; so that an interrupted discharge only can pass through the tube, without dispersing the whole of the water. But if the discharge be not seemingly as strong as the tube can bear without breaking, the gaz is not produced from it; and on this point hinges this extremely delicate process.

The situation of the different parts of the apparatus for the interrupted discharge is shewn by Tab. III. fig. 5.

To succeed by the method of the *complete* or *uninterrupted discharge*, the apparatus now to be described must be used, and the following rules must be observed.

1. A tube, fig. 6. is employed, about four or five inches in length, and its bore one-fifth or one-sixth of an inch in diameter. One end is mounted with a brass tube, fig. 7. and the other end is sealed at the lamp with a wire, about $\frac{1}{40}$ of an inch in thickness, fixed into it, as above described; which extends into the brass tube, so as to be almost in contact where the explosion is made. If the wire touches the brass tube, there will be no gaz produced. The tube being filled with water, and set in a cup of water, the discharge may be made into it, as in the above described process of Mr. VAN TROOSTWYK; but here the insulated ball must be placed at a greater distance from the prime conductor, and a Leyden jar with only fifty square inches of coating

will answer the purpose. In this way of making the experiment gaz is produced by each discharge, in the brass tube; and in much greater quantity, and with much less frequent accidents, and less trouble, than in the former method with the interrupted discharge. But the gaz obtained with this apparatus always contains a large proportion of atmospherical air, on account of the quantity of water and more immediate and extensive communication of it with the atmosphere. By repeated discharges there is an impression made in the brass tube, in the part where the discharge passes through it, and at last a small hole is made in that part. On this account the same mounted tube cannot serve for producing a large quantity of gaz.

2. The other sort of apparatus, invented by Mr. CUTHBERTSON, is represented by fig. 8. At first it consisted of a glass tube half an inch wide, and about five inches in length, mounted at one end with a brass funnel, and inverted in a brass dish; but afterwards the tube was blown funnel-wise at the end, as shewn by fig. 9. The other end must have a wire, about $\frac{1}{40}$ of an inch thick, sealed into it at the lamp; which wire extends to nearly the bottom of the brass dish in which the tube stands.

The exact distance between the end of the wire and brass dish must be found by trials; that which generally answered in my experiments was about $\frac{1}{10}$ of an inch. If it be properly arranged, gaz will be produced at each discharge.

The Leyden jar used with this apparatus, must contain about 150 square inches of coating.

The distance between the insulated ball and the prime conductor, at which the experiment succeeded, was commonly about half an inch.

If experiments be proposed, in which electric discharges must

be passed through water, or other fluids, for even a much longer time than was consumed in performing those referred to, or related in this paper; it may be an object to employ the wind, or perhaps the power of a horse, to turn the electrical machines: the expence of labourers being considerable.

§ 2. Experiments.

From my journal of the numerous experiments, made during the course of nearly two years, I shall select those which will serve to explain the nature of the process, and show the power of the plate electrical machines; and I shall particularly relate those experiments which afforded the most useful results concerning the nature of the gaz obtained.

1. *With interrupted Discharges.*

Experiment A. About 1600 of these discharges, by means of a thirty-four inch single plate electrical machine, in nearly three hours, produced, from New River water taken from the cistern, and which had not been freed from air by the air pump or boiling, a column of gaz two-thirds of an inch in length and one-ninth of an inch wide. On passing through this gaz, between the two wires of the tube in which it was produced, a single electric spark, its bulk was instantly diminished to two-thirds. In other experiments the bulk of gaz was only diminished to about one half. And the result was the same with distilled water.

B. The experiment A being repeated several times, with distilled and New River water, freed from air by the air pump or long boiling, the quantity of gaz just mentioned was obtained in about four hours.

On passing an electric spark through this gaz, in the situation

above mentioned, its bulk was instantly diminished, in some cases $\frac{1}{15}$, and in others $\frac{1}{20}$.

C. 1600 interrupted discharges, by means of a thirty-two inch plate machine, produced, from New River water and distilled water freed from their air by the air pump, a column of gaz about three-fourths of an inch in length, and one-ninth of an inch in diameter, in the space of three hours. It was reduced in bulk $\frac{1}{20}$ by passing through it a single electric spark.

D. 500 revolutions of the thirty-two inch plate machine, in three quarters of an hour, produced 600 interrupted discharges in river water, freed from air by the air pump, by which a column of gaz, half an inch in length and one-tenth of an inch in diameter, was obtained. It was diminished, as usual, by an electric spark, $\frac{1}{20}$ of its bulk.

E. Nearly four days incessant labour, with the thirty-two inch plate machine, produced only 56,5488 cubes of gaz, of one-tenth of an inch each; on account of the usual accidents during the process. The air had been exhausted, by setting the water under the receiver of the air pump.

F. It was found that 6000 interrupted discharges produced about three inches in length of gaz, measured in a tube $\frac{3}{20}$ of an inch in width, from water out of which its air had been drawn by the air pump.

G. It appeared, from many experiments, that the same unboiled water, or water from which the air had not been exhausted by the air pump, which had repeatedly yielded gaz by passing through it electric discharges, always left a residue of gaz, which the electric spark did not diminish; and this residue was in nearly the same quantity, after six or seven experiments, each of which afforded a column of gaz, half an inch in length,

and one-ninth of an inch in diameter, as was left on passing the electric spark through the gaz, afforded by the third or fourth experiment.

Hence it seems, that water is decompounded by the electric discharge, before the whole of the common or atmospherical air is detached from the water, by merely the impulse of each discharge. Yet I think it probable that, after the discharges have been passed through the same water for a certain time, the whole of the air contained in water will be expelled, and no gaz be produced, but that compounded by means of the electric fire from water; in which case, supposing the gaz so produced to be at last merely hydrogen and oxygen gaz, it will totally disappear on passing through it an electric spark. But I have never been able to determine this point; because the tubes were always broken after obtaining a few products, or long before it could reasonably be supposed the whole of the air of the water was expelled from it.

H. To the gaz obtained in the experiment E was added, over water, an equal bulk of almost pure nitrous gaz. Fumes of nitrous acid appeared, and the gaz examined was reduced almost one-third of its bulk. A small bubble more of nitrous gaz being let up no further diminution took place. To this residue was added half its bulk of oxygen gaz, obtained from oxymuriate of potash. This mixture of gazes having stood several days over well burnt lime and boiled quicksilver, an electric spark was passed through the mixture, over quicksilver; by which its bulk was instantly diminished one-fourth. But no moisture could be perceived upon the sides of the tube, or on the quicksilver. The failure of the appearance of moisture was imputed to a bit of lime accidentally left in the tube,

which was burst by the explosion and dispersed through the tube; or else the quantity of water produced was so small, comparatively with the residuary gaz, that the water was dissolved by it in the moment of its composition. For supposing water to have been compounded, it could not amount to the $\frac{1}{100}$ part of a grain; and the residuary gaz was at least two thousand times this bulk.

That a quantity of water can be compounded, under the same circumstances as in this experiment, and be apparently dissolved in air, so as to escape observation, even with a lens, was proved by passing an electric spark through a mixture of hydrogen and oxygen gaz, well dried by standing over lime.

2. *With complete or uninterrupted Discharges.*

The gaz obtained by the first described kind of apparatus, for the uninterrupted discharges, p. 145, and fig. 6 and 7, always left a residue of at least one-fourth of its bulk on passing through it the electric spark; even when water was used, which had been freed from air by boiling, or the air pump. Nor will this result appear surprising, when it is considered how liable the water in this apparatus is to mix and absorb air during the experiment. However, this method would have been extremely valuable if the next other method had not been discovered; for gaz may be obtained by it with fewer accidents, and much more rapidly, than with the interrupted discharges. The apparatus is also much more easily fitted up, and is more simple. But I think it unnecessary to particularly relate any experiments, as they afforded the same results as those already described, and as those next to be related.

The following experiments were made with the apparatus described p. 148, and shown by fig. 8, 9, and 10.

Experiment 1. At $0^h\ 40'$ P. M. began to produce discharges with a double plate twenty-four inch machine, in water taken from the cistern: and at $12^h\ 6'$ P. M. of the same day there had been written down 10200 discharges, each of which occasioned air to ascend from the bottom of the wire and brass cup. The quantity of air obtained was now apparently about one-fourth of a cubical inch, and it occupied nearly half of the tube; the water in which was by this time very muddy.

After standing till the day following at noon, when the process was again commenced, it did not appear that any of the gaz had been absorbed by the water over which it stood.

At $2^h\ 35'$ P. M. began to produce discharges, and at 8^h P. M. had passed 6636; which, together with those of the preceding day, amounted to 16836. The tube was now $\frac{1}{4}$ full of gaz, and there seemed to be almost half a cubical inch; for it was observed, that the gaz was this day yielded at double the rate it had been the day before. This was accounted for from the diminished pressure upon the electric fire, by the tube containing gaz instead of water.

At this time, namely, at 8^h P. M. I was surprised, on the passing of a discharge, by a vivid illumination of the whole tube, and a violent commotion within it; with, at the same time, the rushing up of water, instantly to occupy rather more than $\frac{1}{8}$ of the space which had been occupied by gaz.

The residue of gaz was not diminished further by an electric spark; and to the test of nitrous gaz it appeared to be rather worse than atmospherical air, as it consisted of rather less than one part of oxygen, and three parts of nitrogen or azotic gaz.

It seemed as if the electrical discharge had kindled the oxygen and hydrogen gaz of the decompounded water, by flying from the bottom of the wire to the brass funnel; so that the fire returned into the tube where it passed through the gaz. Or the combustion might be occasioned by a chain of bubbles, reaching from the brass dish to the surface of the water in the tube, which was set on fire in its ascent, and thus produced combustion of the whole of the gaz of decompounded water.

That this phænomenon was from the combustion here supposed, was in some degree proved by finding that the mixture of hydrogen gaz and atmospherical air, under the same circumstances, was kindled in the same manner.

Experiment 11. With a double plate electrical machine, 24 inches in diameter, and a similar apparatus to that in the last experiment, 14600 discharges produced, at least, one-third of a cubical inch of gaz. While I was measuring with a pair of compasses the quantity of gaz produced, the points of them being in contact with the part of the tube occupied by gaz, I was again surprised, on the passing of a discharge, by an illumination of the whole tube, and the rushing up, with considerable commotion, of water, to occupy about two-thirds of the space filled by gaz.

The residuary air was found, as in the former experiment, to be rather worse than atmospherical air.

It was concluded that the points of the compasses had attracted electrical fire from the wire to the sides of the glass, and thereby kindled the hydrogen and oxygen gaz of decompounded water. But to determine this question, I introduced into the same tube a mixture of one measure of oxygen and two measures of hydrogen gaz, to occupy nearly the same

space in the tube as the gaz had occupied: then passing an electrical discharge through it no combustion was excited; but on passing a discharge while the compasses were in contact with the tube, as just mentioned, an illumination and violent commotion were produced, with the rushing up of water, to leave only $\frac{1}{6}$ of the gaz as a residue. On repeating this experiment with one measure of atmospherical air and two of hydrogen gaz, combustion could not be excited; nor with two measures of atmospherical air and one of hydrogen; nor with two measures of hydrogen gaz and one of atmospherical air; but on adding to this last mixture one measure of oxygen gaz, the electrical discharge produced the phænomena of combustion just mentioned, with the rushing up of water, to occupy about two-thirds of the space which was occupied by the gazes.

Experiment III. Having passed 12000 discharges through water, with the apparatus of the preceding experiment, and thereby obtained only one-fifth of a cubical inch of gaz; and having observed, that the quantity of gaz was not greater than it was when only 8000 discharges had been passed, and yet bubbles had been seen to be produced on each discharge as copiously, or more so, by the last 3 or 4000 discharges as before; I began to suspect that part of the gaz had been destroyed during the process, or had been absorbed. While I was considering how to account for this disappearance of gaz, and was at the same time looking at the tube through which the discharges were passing, I observed one of them to be atended with a diminution, instantly, of about one-fifth of the gaz produced, and with a slight commotion. I was now sure, from this phænomenon, and from the unequal augmentation of the bulk of the gaz at given

times during the process, that combustion had been excited several times before; not only in the present experiment, but perhaps in the former ones, without observing it. I conceived that a gradual combustion also, very probably, took place in this process, by the kindling of bubbles of gaz in their ascent through the water. I now perceived that the discharges ought to be produced more slowly, or the tubes to be wider, to allow the bubbles to pass quite through the water, in order to avoid the accension of gaz during the process. My calculation also, that 35 to 40000 discharges were requisite to produce one cubical inch of gaz from water, containing its usual quantity of common air, was rendered much more vague by this accension, so often liable to be occasioned.

To the gaz which remained in the tube in this experiment was added an equal bulk of nitrous gaz; the mixture diminished to 1,5; and on adding to the residue half its bulk of oxygen gaz, and passing through it the electrical spark, no accension or diminution of bulk was produced. Hence all the hydrogen gaz and oxygen gaz, produced by the decomposition of the water, had been burnt during the process; the oxygen gaz thus detected being considered to be only that expelled from the water.

Experiment IV. By means of electrical discharges, with the apparatus used in the preceding experiment, I obtained gaz from New River water; letting it up into a reservoir as soon as about $\frac{1}{10}$ of a cubic inch was produced, till I had collected $\frac{1}{4}$ of a cubic inch. To this was added an equal bulk of nitrous gaz; on which the mixture diminished to 1,2; and on the addition of a little more nitrous gaz, no further diminution took place. To this residue half its bulk of oxygen

gaz was added, and this mixture of gazes being well dried by standing over lime and boiled quicksilver, an electric spark was passed through it, by which a diminution of $\frac{1}{6}$ of its bulk took place. A little dew was then seen upon the sides of the tube where the quicksilver had risen; and, with the aid of a lens, the same appearance was perceived on the part of the tube containing the residue of gaz.

It may now be expected, that I should have made the experiments with this apparatus on distilled water freed from its air, not only by long boiling, or the air pump, but by passing through it several hundred electrical discharges. It would also have been, to some persons, more satisfactory, if the experiments had been made upon a larger scale, so as to have produced the combustion of a much larger quantity of gaz, and consequently have produced a greater quantity of water. As, however, I apprehend, the experiments contained in this paper, when well considered, by competent judges, will be found to explain the nature of the gaz procured from water by electrical discharges; and as another very important subject demands my attention, the honour of more splendid and convincing experiments must be reserved for other inquirers. If the same sacrifices be made by them, which have been made in performing the present experiments, I think it is scarcely possible but that still further light concerning the composition of water should be obtained, as well as concerning oils, alcohol, acids, &c.; to the investigation of the composition of which, the mode of analysis and synthesis here indicated, may be applied.

§ 3.

The following conclusions appear to me obvious and incontrovertible.

The mere concussion by the electric discharges seems to extricate not only the air dissolved in water, which can be separated from it by boiling and the air pump, but also that which remains in water, notwithstanding these means of extricating it have been employed.

The quantity of this air varies in the same and in different waters, according to circumstances. New River water from the cistern yielded one-fifth of its bulk of air, when placed under the receiver of Mr. CUTHBERTSON's most powerful air pump; but, in the same situation, New River water taken from a tub exposed to the atmosphere for a long time yielded its own bulk of air. Hence the gaz produced by the first one, two, or even three hundred explosions in water, containing its natural quantity of air, is diminished very little by an electrical spark.

The gaz or air, thus separable from water, like atmospherical air, consists of oxygen and nitrogen or azotic gaz; which may be in exactly the same proportions as in atmospherical air, for the water may retain one kind of gaz more tenaciously than the other; and on this account the air separated may be better or worse than atmospherical air, in different periods of the process for extricating it.

The nature of the gaz, which instantly disappears on passing through it an electric spark, is shown by

(*a*) This very property of thus diminishing; and by the following properties;

(*b*) A certain quantity of nitrous gaz instantly disappeared, apparently composing nitrous acid, on being added to the gaz (*a*) p. 149, H. 154, Exp. IV.; oxygen gaz being added to the residue after saturation with nitrous gaz, and an electric spark being applied to the mixture of gazes, well dried, a considerable diminution immediately took place, and water was produced; p. 154, Exp. IV.

(*c*) Combustion from hydrogen and oxygen gaz took place, when the tube was about three fourths full of gaz, p. 152, Exp. I. which was confirmed by passing an electrical discharge, under the same circumstances, through a mixture of hydrogen and oxygen gaz, p. 152, Exp. I.

(*d*) Combustion from hydrogen and oxygen gaz took place, when the points of the compasses were accidentally applied to the part of the tube containing gaz, p. 152, Exp. II.; which was confirmed by passing a discharge, under the same circumstances, through a mixture of hydrogen and oxygen gaz, while the points of the compasses were applied to the tube; p. 153, Exp. II.

(*e*) The observations made of the kindling of gaz in small quantities, from time to time, during the process of obtaining it, particularly while it was ascending in chains of bubbles, or was adhering to the funnel of the tube, p. 153, 154, Exp. III. confirm the evidence in favour of this gaz being hydrogen and oxygen gaz.

The evidence contained under the heads (*a*)—(*e*), considered singly and conjunctively, I apprehend, must be admitted

by the most rigorous reasoner, to be demonstrative that hydrogen and oxygen gaz were produced by passing electric discharges through water.

With regard to the origin and mode of production of these two gazes, our present observations and experiments do not afford complete demonstrative evidence; but, although some hypotheses must be admitted, I conceive that the body of evidence we possess can afford a satisfactory interpretation of the phænomena.

EXPLANATION OF THE PLATE (Tab. III.)

Fig. 1, 2, 3, 4. represent the tubes used in producing gaz from water by the interrupted electric discharges.

Fig. 5. represents the situation of the above tubes during the process of producing gaz from water.

Fig. 6, 7. represent the tubes employed in producing gaz from water by the first method, with uninterrupted electric discharges.

Fig. 8. shows the figure of the tube mounted with a brass funnel, used in the second method of producing gaz from water by uninterrupted electric discharges.

Fig. 9. represents the tube blown funnel-wise at the end, instead of being mounted with a brass funnel, as in fig. 8.

Fig. 8. represents the situation of the tubes fig. 8. and 9. during the process of producing gaz by the uninterrupted electric discharges.

Fig. 1.

Fig. 2.

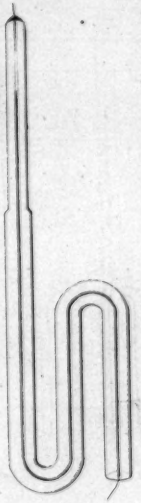


Fig. 3.

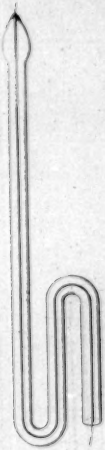
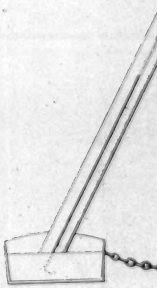


Fig. 4.



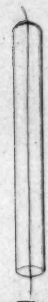


Fig. 6.

Fig. 7.

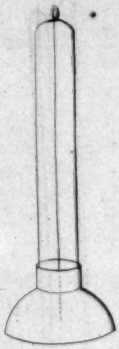


Fig. 8.

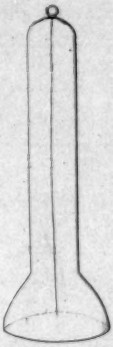


Fig. 9.

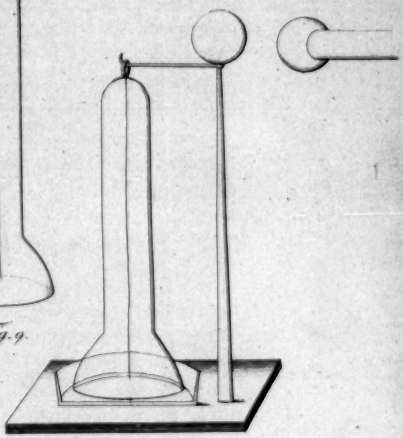


Fig. 10.

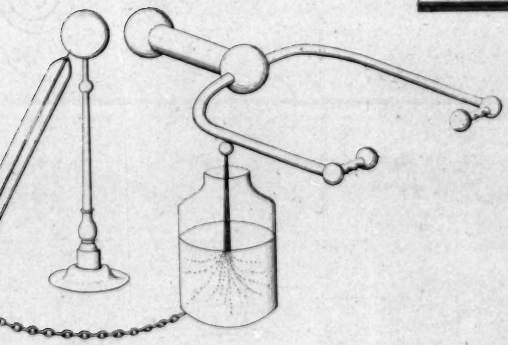


Fig. 5.

VIII. *An Experimental Inquiry concerning Animal Impregnation.* By John Haighton, *M. D.* Communicated by Maxwell Garthshore, *M. D. F. R. S.*

Read February 2, 1797.

Difficillimum aggredior laborem, et exitum vix promitto qui lectori satisfaciat.

This was the sentiment of the justly celebrated Baron Haller, when he first directed his attention to this subject, when he attempted to produce order and regularity out of chaos, and to show

"How the dim speck of entity, began
"To extend its recent form, and stretch to man."

Garth.

The difficulties which discouraged so able a philosopher, are but ill calculated to inspire me with confidence; but the disappointment from failure will be attended with this solacing reflection, that if I have miscarried, it is in a great undertaking.

The multitude of physiologists who have sought for laurels in this field, can best bear witness to the difficulty of the pursuit; and the penetrating genius of a Harvey, though adequate to a full exposition of the circulation of the blood, toils in vain in the mysterious researches of generation. His philosophic

mode of scrutiny by experiment, when pointed to one object, conferred immortality on his name; but when directed to another, reduced him to a level with contemporary reputation.

Others, perhaps from possessing a greater propensity to the subject, have laboured with more success: they have penetrated into the interior recesses of nature, and thence brought to view what preceding investigators had deemed inaccessible to research. On this view of the subject, our acknowledgments are particularly due to the labours of STENO, DE GRAAF, HALLER, and others. To STENO and DE GRAAF we are indebted for some important facts on the structure of the ovaries. The supposed analogy to the male's testes is disproved, and the vesicular structure, together with a connexion with the ova, or rudiments of the new formed animal, fully established.

From the experiments of DE GRAAF on rabbits, we learn,

First. That the ovaries are the seat of conception.

Secondly. That one or more of their vesicles become changed.

Thirdly. That the alteration consists in an enlargement of them, together with a loss of transparency in their contained fluid, and a change of it to an opaque and reddish hue.

Fourtbly. That the number of vesicles thus altered, corresponds with the number of fœtuses, and from these are formed the true ova.

Fiftbly. That these changed vesicles, at a certain period after they have received the stimulus of the male, discharge a substance, which being laid hold of by the fimbriated extremity of the fallopian tube, and conveyed into the uterus, soon assumes a visible vesicular form, and is called an *ovum*.

Sixtbly. That these rudiments of the new animal, which for a time manifested no arrangement of parts, afterwards begin to

elaborate and evolve the different organs of which the new animal is composed.

To these facts we may add, that the *calyx* or *capsula* which formed the parietes of the vesicles, thickens, by which the cavity is diminished. This cavity, together with the opening through which the fœtal rudiments escaped becomes obliterated, and from the parietes of these vesicles having acquired a yellowish hue, they are called *corpora lutea*.

But though some important facts are clearly ascertained, there are others still problematical. Physiologists are by no means agreed concerning the *immediate cause* of conception. All admit the necessity of sexual intercourse. They acknowledge too the necessity of some part of the female being affected by the direct contact of a fecundating fluid, but what the precise part is which must receive the stimulus, has hitherto been involved in mystery and doubt. Nor are they more unanimous respecting the state or condition of the substance that passes from the ovaries; whether at the time of its expulsion it has a circumscribed vesicular character, or whether it has no determined figure. De Graaf and Malpighi, in the last century, and some respectable physiologists of the present day, adopt the first opinion; Haller and some others favour the last.

The subject of conception involves other problematical points not less interesting; the discussion of which I purpose waving at present, in order the better to direct my attention more closely to the preceding questions.

The intention then of this essay is to explore the *proximate cause* of the impregnation of animals, and to trace with more accuracy the visible effects of it from their first appearance, until the rudiments of the fœtus are lodged in the uterus, and

have assumed the proper characters of an *ovum*. As soon as these rudiments manifest that opaque spot, or "dim speck of "entity," which is known to evolve the fœtus by regular and progressive steps; another stage of the inquiry then commences, viz. to trace the visible formation of the new animal through its whole course; but as this belongs rather to the œconomy of the fœtus than the mother, it is not intended to form any part of this paper.

I perceive, however, that I cannot investigate the question of the proximate cause of impregnation in a satisfactory way without first determining what are the evidences or proofs that impregnation has taken place: this then necessarily becomes a preliminary question. I therefore restrict my inquiry to the three following subjects.

First. What are the *evidences* of impregnation?

Second. What is the *proximate cause* of impregnation?

And, *third.* Under what form do the rudiments of the fœtus pass from the ovary to the uterus?

SECTION I.

What are the Evidences of Impregnation?

The investigation of every complicated subject of inquiry comprehends within its range a more or less extended recital of facts, depending in a greater or less degree on each other, but primarily arising from some fundamental proposition.

As this proposition is generally the basis on which this superstructure is raised, or the trunk from which the various ramifications of inquiry proceed, it is essential, to the establishment of the ultimate conclusions, that the antecedent question

be rightly decided. It becomes then indisputably necessary to us in the present subject, to determine *what is the criterion of impregnation.*

That a female is impregnated when a fœtus is sensibly formed, is so obvious to reason that no argument can be necessary to convince us of its truth. But it is important to some conclusions in the sequel of this paper to prove, that a female has conceived before there are any vestiges of a new animal. The test of this condition must then be sought for in the ovaries; and the well conducted experiments of De Graaf, in the last century, and of Baron Haller and others, in the present, bear so forcibly on this point, that the necessity of further investigation is in a great measure precluded.

But, in order that I might bear evidence of its truth, I examined with great attention the ovaries of some full grown virgin rabbits, and found, as De Graaf has represented, that there entered into their composition a series of cells containing a transparent colourless fluid. It was indispensably necessary here to be certain, that these rabbits had never been admitted to the male, lest the remains of former impregnations should be confounded with virgin appearances. I therefore observed with care not only the appearance on the surface of these bodies, but likewise examined with great minuteness the interior parts; yet in none of them could I see any of those circumscribed substances, which, from their yellow colour, are called *corpora lutea.* But when similar observations were made on rabbits that had been impregnated at different periods, and the traces of those corpora lutea were more or less evident, according to the interval of time that had elapsed; I may then say that no corpora lutea exist in virgin animals, and that when-

ever they are found, they furnish incontestible proof, that impregnation either does exist, or has preceded.

But a proper distinction between past and existing impregnation can be made only by tracing the phænomena of recent fecundation progressively, and noting the appearances in the different stages. I was therefore under the necessity of repeating with care several of DE GRAAF's experiments, in order that I might bear testimony to the truth of them, at least as far as the results coincided with my own.

EXPERIMENT.

Having therefore procured several virgin rabbits in a fit state for impregnation, I admitted one of them to the male. Twelve hours afterwards it was killed, and on examining the ovaries several of the vesicles evidently projected; they had lost their transparency, and were become opaque and red. When punctured, a fluid of the same colour escaped. I made sections through some of them; but at this early period the corpora lutea, which are formed by the thickening of the parietes of the vesicles, were not very evident. I therefore determined to examine them in a more advanced state.

EXPERIMENT.

Another rabbit being admitted to the male, I examined it twenty-four hours afterwards. The colour of the fluid contained in the vesicles was similar to that of the last experiment. The vesicles projected more evidently, and their thickened parietes manifesting the commencement of corpora lutea were become more apparent.

EXPERIMENT.

I inspected the ovaries of another rabbit forty-eight hours *post coitum*. At this period the vesicles seemed to be in the very act of bursting, and a semitransparent substance, of a mucus-like consistence, was beginning to protrude from some of them; others indeed were less advanced. The fimbriated extremities of the fallopian tubes were preparing to receive their contents, as appeared by having quitted their usual position, and embraced the ovaries in such a degree, that only a small portion could be seen until the tubes were taken away. Sections being made into the thickened vesicles, the formation of corpora lutea appeared to have made further advances.

From the appearance of an incipient rupture of the vesicles in this experiment, it was but reasonable to expect that their contents would soon have escaped; but as my views were directed to the formation of a corpus luteum, I deferred the next examination to a more distant time.

EXPERIMENT.

In two days and twelve hours after coition, I examined the ovaries of another rabbit. The fœtal rudiments had escaped; but the cavity of the ovarian vesicles had suffered but little diminution. Bristles were easily introduced by the ruptured orifices. In this experiment the advances towards the formation of a perfect corpus luteum were such as the period of examination would naturally lead us to expect.

The contents of the vesicles having escaped, it was but reasonable now to look forward to a speedy obliteration of the

cavity. I therefore examined these parts under similar circumstances on the third, fourth, and fifth day. In the last experiment there was but little vestige of cavity, consequently the corpora lutea might be considered as perfectly formed.

I think it not improper to remark here, that though in the relation of the above experiments I have constantly kept in view the formation of corpora lutea; yet I did not altogether neglect the opportunity of making other observations, which in this early stage of the inquiry it would be premature to relate. Besides which, several other rabbits were examined at more distant periods, as well with a view of tracing their progress with accuracy, as to afford further evidence of their connexion with impregnation. But as it would be tedious to state in detail the several experiments made on this single question, by reason of the great similarity of result, I decline trespassing on your patience, and therefore lay before you only the conclusion; which is, that in the great variety of experiments on brute animals which my physiological inquiries have led me to conduct, as well as in the extensive opportunities I have had of observing the ovaries in the human subject, I have never seen a recently formed corpus luteum unattended with some circumstance or other connecting it very evidently with impregnation. I have more than once seen a recently formed corpus luteum in the human subject, *without* a fœtus. Nay, even in a subject where there has been a kind of *hymen*: but the uterus in these cases has borne the marks of an early and recent abortion.

SECTION II.

What is the proximate Cause of Impregnation?

The preliminary question concerning the criterion of fecundation being now answered, we are led by a natural transition to show by what means this test has been produced.

Waving all comment on the peculiar circumstances of sexual intercourse, as being both irrelevant and indelicate, we shall note only one important effect of it, the passage of the fecundating fluid of the male into the generative organs of the female, as being an indispensable requisite in the human female, and in such animals as bear an affinity to it. As this effect of sexual communication is so important, it cannot be indifferent to the design of nature, to what part of the uterine system the semen should be conveyed. It admits of no doubt that it either remains in the vagina, passes into the uterus, or else extends its course along the fallopian tubes to be applied to the surface of the ovaries, which it stimulates, and from which the new animal derives its existence; but whether it be one or other of these, has given birth to more physiological controversy, than perhaps any other operation of a living animal.

Those who have entered the lists have ranged themselves either on the side of application of the semen to the ovaries by means of the tubes; or on that of the inutility of this process. These latter contend for an absorption of this fluid by the vagina, a peculiar excitement of the whole frame as a consequence, of which excitement the changes produced on the ovaries are to be considered the local effects. But though the question has been disputed on both sides with all the zeal of argument

and controversy, the arbiters of science have not yet acknowledged a victor on either side.

The advocates for the first opinion allege, that the semen has been seen both in the uterus and tubes, and quote as their authority the observations of MORGAGNI for the former, and RUYSCH for the latter. When seen in this last situation, some have thought that it was conveyed thither by the muscular power of these parts in the manner of a peristaltic motion, beginning at the uterus and ending at the fimbriated termination of the tube; and when at this last, it was supposed that the semen was applied to the surface of the ovaries, and impregnated them by actual contact.

Though I shall prove that this hypothesis is altogether visionary, yet *prima facie* it is far from carrying with it the characters of absurdity. There is nothing repugnant to reason in contending for what analogy seems to favour, particularly when the subject is thought beyond the reach of demonstration or proof. And the analogy favourable to this opinion has probably been taken from the impregnation of frogs and toads, in which process we are told, on the authority of ROESEL, SWAMMERDAM, and SPALLANZANI, the ova are impregnated by the male as they are passing from the body of the female; and that in water newts the ova are impregnated even without copulation. Now here is an appearance of contact between the fecundating fluid and the ova.

Again, on the other hand, the contact of semen with the ovaries has been thought improbable, from an analogy drawn from the vegetable kingdom; for admitting the Linnæan doctrine to be true, which contends for a necessity of sexual intercourse in vegetables, it would be difficult to demonstrate to

the satisfaction of stern philosophers, that the *pollen* pervades the *pistillum*, and stimulates the contents of the *pericarpium* by contact, to the evolution of the *germen*. Such would deny the contact of semen. The advocates for either opinion then may avail themselves of analogies suited to their own mode of thinking. It may be said, however, and with some colour of truth, that the latter analogy, as being more remote than the former, and as being founded on a principle which some have suspected to be gratuitous, should be received with caution and distrust. Before any deduction can be made from analogy concerning the means by which any important end is to be effected, we cannot examine the instruments performing such actions with an attention too nice or too minute. If we find nature employing different instruments, in different animals, to produce the same ultimate effect, I think it but fair to conclude, that the means used are essentially different; but the closer the resemblance in the instruments or organs, the nearer will the means approach. On this principle no conclusions can be drawn respecting the human species, from observations either on vegetables, or even on frogs, toads, and newts. Birds, as being impregnated by semen conveyed into the body, resemble human impregnation more than the former; but they differ so obviously in the mode of perfecting the fœtus from the ovum, that I scarcely dare to rest any thing on their general analogy. There is, however, a curious fact respecting them not altogether inapposite to this question, which is, the permanent effect of one coitus. I have read in the Abbé SPALLANZANI's dissertation, and elsewhere, that all the eggs which a hen will lay in twenty days will be impregnated at one coitus: and Mr. CLINE tells me, that in Norfolk this matter is reduced to a certainty

with respect to turkeys; and that even to a greater extent. There is certainly some difficulty in reconciling these facts to impregnation by contact of semen; but from the very obvious difference between oviparous and viviparous animals, I shall not press this argument farther. Indeed it should always be impressed on the recollection of those who are labouring in the pursuit of truth, that arguments drawn from analogies, unless from those of the nearest relation, are better adapted to the purpose of illustration than of proof: and though they frequently find advocates in confident closet philosophers, they are received with deserved distrust by the more cautious practical physiologists.

Those who cannot admit the passage of semen by the tubes, do not neglect to take the advantage of some difficulties which their opponents have overlooked. They say, implicit confidence is not due to the observations of MORGAGNI and RUYSCH, and that what appeared to them to be semen in the uterus and tubes, was nothing more than the mucus of the parts. They further invalidate the force of this argument by contrasting these solitary observations, with a numerous train of counterfacts; for in all the experiments made by HARVEY, DE GRAAF, HALLER, and others, it does not appear that semen was found beyond the vagina, except in one of Baron HALLER'S experiments in a sheep, in which he saw semen in the uterus forty-five minutes after coition. But this fact stands almost alone; and when placed in opposition to the many experiments attended with a contrary result, will weigh but little in the balance of impartial decision. Yet, however, he rested much upon this one fact, and adduced it in support of his opinion, that whenever impregnation happened, the semen passed into the uterus,

and was retained; but when it returned from the vagina, then the animal remained unimpregnated. In this latter case, he supposes the semen had never passed beyond the vagina; for if it had, he says it would have been retained. This argument he thinks is unanswerable.

The insufficiency of this reasoning did not escape the penetration of his opponents; and the immense mass of counterfacts poured out against him, like an irresistible torrent, bore away the very foundation of his doctrine. This brings the advocates for the necessity of the contact of semen with the ovaries into a dilemma, from which they attempt to extricate themselves by contending, that fecundation does not require the application of semen to the ovaries in a palpable form; but that there is exhaled from it a subtile fluid in a vaporific state, called *aura seminalis*, and that the contact of this vapour is fully sufficient to impart to the ovaries their due quantity of stimulus.

But the opinion, even thus qualified, has not passed without animadversion. There are some who cannot comprehend how the tubes should perform two motions in contrary directions, which they must do, if they first convey the aura seminalis to the surface of the ovaries, and afterwards return the rudiments of the fœtus into the uterus. Such a double action they think is repugnant to the œconomy of the part, but assign no reason for their opinion. They might with equal propriety deny the possibility of a peristaltic and inverted peristaltic motion of the intestines, or the opposite actions in the œsophagus of ruminant animals, though I am persuaded very few would acquiesce in their incredulity: but as a minute discussion of this particular

question would be rather extraneous to my investigation, I must decline any further disquisition.

The difficulties which were opposed to the conveyance of the semen by the tubes, were, as we should suspect, intended to prepare the way for a different explanation; therefore physiologists, by a very natural transition of thought, were led to suppose that the presence of semen in the vagina alone was sufficient to account for impregnation.

In order to give support to this opinion, cases were adduced, in which, from some anatomical peculiarities, it seemed almost impossible that the fecundating fluid could be conveyed into the uterus; and yet in several of these cases impregnation had really taken place. It would be digressing too much to state the facts in detail, seeing that in this inquiry I deduce nothing from them; nor would such statement solve the problem before us. The facts are already in the possession of physiologists, but are not admitted as satisfactory proofs. Those who hold the contrary opinion, either cavil at the accuracy of the statement, or draw a different conclusion; therefore to attempt conviction by these materials would be to engage in the service of forlorn hope. It remains then to try whether by a patient experimental investigation, we can make such an accession of new facts to our present stock of knowledge as will enable us to unloose this Gordian knot. This attempt naturally leads us to review the two points of the question, viz. *Is the passage of the semen by the tubes to the ovaries, essential to impregnation? If not, what other means are employed?*

If it be true that the fecundating fluid must pass by the tubes to the ovaries before impregnation can take place, ought it not

to follow, as a consequence, that if, from any cause, both these tubes be obliterated, the animal so affected would be barren? or if the animal be multiparous, would not an obliteration on one side prevent conception in the corresponding ovary?

Now I had some distant apprehensions, even before I made this experiment, that dividing both tubes would produce effects equivalent to an extirpation of both ovaries, which experience has since proved to be well founded; for it not only destroys the power of conception, but even the disposition for using the means.

EXPERIMENT.

Having procured a full grown virgin rabbit, which had betrayed signs of disposition for the male, I made an incision into the posterior part of each flank, exactly upon the part where the tubes are situated. By means of my finger and a bent probe, I drew out a very small portion of the middle of the tube, and cut out about $\frac{1}{4}$ of an inch. The two ends were returned into their former situation, and the wound closed by what surgeons call the quill suture. The same operation was performed on the opposite side, and in a few days both wounds were healed.

As soon as this rabbit appeared in health, it was admitted to the male, but the venereal appetite seemed to be entirely lost. Thinking it possible that its health was not perfectly restored, I kept it a month longer in a state of high feeding, and admitted it to the male a second time, but the same reluctance continued. I began now to suspect that the venereal appetite was irrecoverably gone: but as the season was cold, and of course unfavourable, it appeared proper to persevere in this plan until the

genial influence of returning spring had produced its effect; but instead of discovering signs of the restoration of the female character, it was evidently more averse. It was now killed and examined, the tubes adhered firmly to the loins at the part where they were divided, and at that part their canal was obliterated, so that neither quicksilver nor air could be made to pass. The ovaries were much smaller than they usually are in breeding rabbits; they appeared to have degenerated from their proper character, a circumstance probably the consequence of that destruction of the harmony of action in these parts, which subsists in the healthy state, which is essential to the views and intentions of nature, and for want of which harmony, the sexual indifference, approaching to aversion, was in this instance so remarkable.

In the relation of this experiment, it must be remembered, that a small portion of each tube was cut out, in order to obliterate the canal with greater certainty. It is not altogether indifferent to the present subject to know, whether this apathy depended on the removal of that portion, or whether it would have happened had there been nothing more than a mere division. Nor is it extraneous to inquire, whether a simple division of the tube is sufficient to obliterate it, because less violence is offered to the part, and of course the connection will be less disturbed.

EXPERIMENT.

Being furnished with another rabbit, in high breeding condition, I repeated the experiment, by making only a division of the tubes; in other respects every thing was conducted as before. The venereal appetite declined as evidently in this as

in the former, and notwithstanding many solicitations from a very animated male, during the space of three months, it could never be excited.

On dissection, it appeared that the tubes were as completely obliterated in this experiment as in the last, and the ovaries had equally degenerated.

In the two preceding experiments neither of the rabbits had given any active proofs of fecundity, though they had marks of the venereal heat upon them. I therefore changed my subject for one that had had young ones.

EXPERIMENT.

A healthy rabbit, which had lately been separated from her first litter, was made the subject of a repetition of the experiment. I took the opportunity of feeling for the ovaries, in order to have better evidence respecting their bulk, and by that means to form a juster comparison. The disposition to propagation declined as evidently in this animal as in the two former; and dissection equally evinced a change of the ovaries; for at the expiration of three months, they had lost nearly half their size.

Feeling but little encouragement to persevere in a repetition of these experiments, I determined to change the mode of inquiry, and to try the effect of a division of *one* tube only. From reasoning I was led to think, that if a division of both tubes destroyed the harmony of the generative system, a division of one only might permit that harmony to continue in some degree. I wished likewise, if possible, to have this point determined on a virgin rabbit, the better to guard against any

deception which the remains of a former impregnation might occasion.

EXPERIMENT.

A full grown virgin rabbit had one of the tubes divided at a little distance from the extremity of the cornu uteri. The wound soon healed up, and its health was soon restored, but it betrayed no disposition for the male. I attributed it in part to the coldness of the season, for it was in the middle of December, 1794; but the effects of its inclemency were much moderated by having a fire in the room during the day. I kept her until the first of May; during this interval the male was frequently offered to her, but she always refused, except once in February: it however was unproductive.

From examination after death, it appeared that the divided tube was completely obliterated, but the other was sound: both ovaries were evidently shrunk, proving, in addition to my previous observations, that their actions had been languid.

The result of this experiment disappointed me much; for no reasoning *a priori* had led me to entertain the smallest suspicion that a mutilation of one side only could destroy the harmony of the whole uterine system. But my disappointment originated chiefly from the apprehension that this effect would be uniform, that it was the result of a determined law of the part; and if so, it formed an insuperable obstacle to my research. Its importance to my project was too great to be discouraged from a single obstacle; therefore in justice to my undertaking, I was in some measure compelled to push the inquiry to such an extent, as should enable me to say with precision, whether

it is possible to impregnate an animal in the situation just described.

EXPERIMENT.

Two other rabbits full grown and perfectly healthy were made the subject of a repetition of the last experiment. The male was offered to them several times during the space of three months. They generally refused him, yet received him twice or three times each during this interval; but neither were impregnated. As the signs of degeneracy from their proper sexual character became daily more evident, they were devoted to anatomical inspection, and exhibited appearances in the ovaries like the former, but somewhat less in degree.

The rabbit keeper informing me that those which had already had a litter were more certain of breeding than those which had not; I determined to make a trial of one of this description, with a view to compensate for my former disappointment.

EXPERIMENT.

Being furnished with one of this kind, and from which the young had been taken away three weeks at the age of ten weeks, which, together with the month of gestation, amounted in the whole to four months from the last conception, I made this the subject of the experiment. Now, at this distance of time, it is not very probable that the ovaries should retain very evident vestiges of the preceding conception: but as it was a point of too much importance to be left in doubt, I determined to satisfy myself by ocular examination, which, by a little management, was effected. The traces of corpora lutea were

far from being evident, so that there was no danger of confounding them with any recent mark that might happen. The tube on one side was cut through as before, but to my unspeakable mortification this rabbit was as barren as the former, though tried several times during the space of three months. The generative organs were examined after death, and the appearances corresponded with those of former experiments.

In this case, as well as in a former, I had an opportunity of comparing the shrunk state of the ovaries after death, with the plump and healthy condition before the mutilation; and it affords an additional proof of that sympathetic connexion, or consent, between one part of the generative organs and another; and shows that in the production of a new animal, the co-operation of different parts is necessary; and further, that if the assistance of one part is wanting, the others, as if governed by a principle of intelligence, cease to continue their important work. But I was still in a state of suspense with regard to the end for which these experiments were instituted; and such an uninterrupted succession of failures on a point so essential to my present inquiries, I confess tended but little to animate me in the pursuit. I was beginning to suspect that the barrenness consequent to the division of only one of the tubes, was as determined a law in the œconomy of these parts, as it seemed to be in those cases where both tubes were cut through; and that nothing could prevent this sterility; but my contemplations were directed into another channel by the following experiment.

EXPERIMENT.

Having procured another rabbit, nearly under the same circumstances as the last, I operated precisely in the same mode, and had equal evidence too concerning the condition of the ovary. The result of this experiment was successful; for on admitting the male to her about one month from the operation, she betrayed no reluctance, and became impregnated. Ten days afterwards she was killed, and opened. Both ovaries retained their primitive plumpness, and manifested the evidences of impregnation. These evidences are the presence of corpora lutea, bearing the same precise characters as I have demonstrated in the former part of this essay. Those seated in the ovary of the mutilated side did not differ in any respect from the same bodies on the perfect side: but they were unattended with fœtuses; whereas in the perfect side, there were as many fœtuses as corpora lutea

As this experiment had succeeded, I examined the divided tube with attention, to satisfy myself whether its canal was obliterated; and of this I had the clearest proof; for it would not allow quicksilver, nor even air to pervade it.

Now here is matter for reasoning. Both ovaries, it seems, bear unequivocal proofs of impregnation, but fœtuses are found only on one side.

Now, on what principle shall we explain these phænomena? It is certain that neither semen nor the aura seminalis could have touched the left ovary, and yet it bears the most unequivocal marks of recent impregnation. It must depend on some other cause than the actual contact of semen.

But an important subject for investigation here presents

itself. Why were there no fœtuses on the mutilated side; but only the corpora lutea? Is the application of the semen to the vagina or uterus sufficient to stimulate the ovaries to perform their first procreative operations, without enabling them to achieve any thing more? and does it require the permanent and active energies of this fluid, operating by direct contact on the surface of the ovaries, to produce the full measure of their effects? But as these are queries which cannot be answered from the mere reflexions of the closet, I must engage anew in the business of experimental inquiry. But the first step that ought to be taken in the management of this question, is to give full confirmation to the above fact, by a repetition of the experiment; I therefore engaged a keeper of rabbits to procure me six in high breeding condition, as soon as possible.

EXPERIMENT.

Within the space of a month, I cut through the fallopian tube on one side in six rabbits. The season was warm, and consequently favourable for breeding. As soon as they recovered they were admitted to the male: but out of this number two only were impregnated; and the keeper assured me that one of them had never been impregnated before. When the success in these experiments is compared with that of the former, there was no cause for complaint. Of these two which succeeded, one had three corpora lutea and three fœtuses in the perfect side, with two corpora lutea and *no* fœtuses on the imperfect side. The other, which was the virgin rabbit, had two corpora lutea and two fœtuses on the perfect side, with one corpus luteum and *no* fœtus on the mutilated side.

Having now three indisputable proofs of this important fact,

I consider it a full answer to any objection that can be urged on the ground of accidental appearance; and that what has been stated above, must, under the circumstances described, be considered as a law of the part; viz. *That the ovaries can be affected by the stimulus of impregnation, without the contact either of palpable semen, or of the aura seminalis.*

But I cannot expect that any physiologist, prepossessed with the common notion of the contact of semen, will yield assent to my position, without subjecting it to a severe scrutiny, and exposing every possible objection to which it is liable. It certainly would not be unphilosophic to ask, why fœtuses were not found either in the ovarium, or in the tube between it and the obliterated part, agreeably to the assertion of NUCK, if, as I contend, the ovary was affected by impregnation? Again, a tenacious opponent might further avail himself of this apparent difficulty, by alleging that if the tube had not been obliterated until after coition, the semen or its powers might have affected the ovary by actual contact; and the product of conception might have been more complete. And in support of this idea, he might adduce the result of an experiment said to have been made by NUCK, in which he made an extra-uterine case in a bitch, by tying one of the tubes three days after coition.

These objections have at least speciousness to recommend them to our notice; but it is from experiment alone that we can determine whether they have any solidity.

To the first difficulty I reply, that my experiments were not made under the same circumstances that NUCK's is said to have been; therefore, giving him full credit for what he has advanced, a similarity of result cannot be expected. But it is painful to me to differ from any writer of character in the statement of a

fact, where the truth is equally accessible to us both; and notwithstanding the respect I willingly bear towards a name that has both acquired and deserved considerable reputation, I must confess that it appears to me highly problematical, whether this celebrated experiment be a reality, or only an ingenious device. But some facts, which it will soon be in order to relate, will show (I think very clearly) that I rest my suspicion upon fair grounds. In the mean time I feel it incumbent on me to reply to the general principle of the objection, and to determine by experiment how far it is deserving attention.

Now, if there be any validity in the objection, it should necessarily follow, that if an opportunity was given for the semen to pass by the tubes to the ovaries; we might, by opening an animal at a proper time after coition, detect some disposition in the fimbriated extremities of the tubes to apply the semen, by first approaching, and afterwards embracing the ovaries; and this action ought, according to the common theory, to take place before the usual sign of conception is at all evident on those bodies, which in the rabbit is somewhat apparent in six hours, but unequivocally marked in twelve.

Again, admitting the probability of it, we are led to inquire by what power the semen can be conveyed to such a distant part. It must be either by the male, *vi jaculationis*, or by muscular power in the tubes, analogous to a peristaltic motion. If it were by the first mode, the conveyance would be instantaneous; but in the latter, some little time seems necessary to allow the tubes to be affected by the stimulus preparatory to their peristaltic action. Perhaps this question may receive some light from the sacrifice of a few animals, at different periods between the coitus and the first visible effects of impregnation;

and I considered it by no means inapposite to the subject, to determine whether these conjectures were authorized by any visible changes, either in the condition or situation of the tubes. But the fruits of this inquiry will appear by the following experiments.

EXPERIMENTS.

A female rabbit in high season was admitted to the male, and in a few minutes afterwards the ovaries and tubes were brought into view; but the fimbriæ were in their natural situation.

As soon as proper rabbits could be procured, I repeated this experiment on two others, with precisely the same consequence.

These facts militate strongly against the possibility of the conveyance of the semen to this part *vi jaculationis*, and demonstrably prove, as far as three facts can go, that if the moving power inheres in the female, it is not instantaneously exerted.

But are the powers of the fecundating fluid conveyed *at any time* by the tubes?

This simple question betrayed me into the prosecution of experiments to a greater extent than I at first expected; for the result of several of them was unsatisfactory: but being once engaged in the question, I felt myself compelled to prosecute it, by examining these parts at different periods from the coitus to the manifestation of its effects. But I found from a regular series of observations made on different rabbits, at every hour between the first and the ninth, that the fimbriæ remained nearly in their usual situation; and the only differ-

ence I perceived in the last hours, was a greater turgescency of vessels, as if preparatory to some important action. I desisted from this inquiry at the ninth hour, because the ovaries now bore very evident marks of impregnation; and there appeared to have been no action in the tubes by which the semen could have been conveyed to them.

The impression which these experiments at first made on my mind, was, I must confess, not altogether incongenial to my wish, in as much as they seemed to furnish a satisfactory answer to the question; but reflexions when more at leisure abated my confidence, and in the end convinced me that my proofs did not exceed probability, so that there was still room for the suggestions of scepticism: and indeed it might be said with great propriety, that the tubes might have inclined towards the ovaries in the intervals of the hours above mentioned, and have returned to their former situation, and thus have eluded my research. I think it but candid to acknowledge, that these last experiments do not prepare me to meet that objection.

These reflexions suggested to me the expediency of constructing a plan of inquiry more apposite to the subject; and attended with experiments bearing more directly on the point at issue. Under this impression I determined to obliterate one of the tubes at different periods *post coitum*, and after the lapse of a sufficient length of time, to notice the effect. My particular view in this was to allow sufficient time for the arrival of the semen at the ovaries, supposing it to take place; so that if they were stimulated by an affusion of that fluid, either in a palpable or insensible form, here would be time allowed sufficient to produce its effect; and if in this mode fœtuses could

be formed, while by obliterating the tube *ante coitum* nothing more than corpora lutea were seen, it furnished an argument of no inconsiderable force in favour of impregnation by immediate contact; but if on the contrary, corpora lutea ONLY were found, then such experiments would give additional force to the arguments stated in a former part of this section.

EXPERIMENT.

One of the tubes of a rabbit was divided half an hour *post coitum*, and the wound closed as before. She was kept a fortnight, that I might know the result; but there were no marks of impregnation on either side.

Though a failure of impregnation has been very common in experiments connected with the mutilation of these parts, I apprehended that the derangement in the present instance proceeded from some disturbance given to the procreative operations in their commencement, and therefore determined in the next trial to wait a few hours, the better to avoid this.

EXPERIMENT.

I repeated the operation on two other rabbits, in one at four, and in the other at six hours after coition. On inspecting the parts at the end of a fortnight, the first was not impregnated, but the last was. In this there were four corpora lutea in the right side, answering to the same number of fœtuses in the cornu uteri of that side; but on the left or imperfect side, there were three corpora lutea without fœtuses. The corpora lutea on both sides were cut open, but not the slightest difference could be detected.

Now, if the contact of the semen with the ovaries in any

form be essential to impregnation, here has been an opportunity for such contact during the space of six hours; but it has not been sufficient to advance the procreative operations further than happened in those experiments where the tube had been divided before coition. Let us then for a moment suppose that the interval be lengthened, in order to allow a better opportunity for producing the full effects of impregnation, by exposing the ovary a longer time to the stimulus of the semen.

EXPERIMENT.

I cut through the left tube of another rabbit twelve hours *post coitum*, and examined the parts on the fifteenth day. There were four corpora lutea with the same number of fœtuses on the right side, and three corpora lutea without fœtuses on the left; so that twelve hours supposed exposure to semen, had made no sensible advances in the procreative operations on the mutilated side.

EXPERIMENT.

The same operation was repeated twenty-four hours *post coitum*. Corpora lutea were found in both ovaries, but fœtuses only on the perfect side.

Now I observed in one of the experiments related in the former part of this essay, that the vesicles of the ovaries when examined forty-eight hours *post coitum*, were extremely prominent; they appeared as if going to burst: it is but reasonable then to admit, that at this time they must have received their full measure of stimulus; and if one of the tubes was divided in this state of things, the result would be more decisive.

EXPERIMENT.

The operation was repeated under the circumstances just described, and in fourteen days the result was ascertained, viz. three corpora lutea and as many fœtuses on the perfect side, and two corpora lutea without fœtuses on the imperfect one.

Now, what mode of reasoning ought we to adopt here? Has the mutilating process suspended the effect of that stimulus which impregnation had begun? and are those appearances in the ovaries, any thing more than incipient relapses into evanescence? Such really appears to be the state of things, and seems to mark in a decided manner, a sympathetic connexion between one part of the uterine system and another. And were I to adopt the language of a late celebrated physiologist, I should say "that the ovary on the imperfect side, "feeling the inability of the tube to transmit its contents to "the uterus, the proper receptacle, had suspended the usual "operations of these parts, from a consciousness of their in-"utility."

This reasoning will probably appear not perfectly consentaneous to certain well established facts on the subject of extra-uterine fœtuses; for dissection has fully evinced the possibility of a fœtus being perfectly evolved, and of acquiring considerable bulk, either in the ovary, abdomen, or tube.

I do not hesitate to acknowledge the full force of these facts; but I cannot admit that they subvert the principle I wish to establish from experiment; because I conceive there is an essential difference whether nature spontaneously dispenses with her usual modes, and attempts to effect her ultimate purpose by irregular means; or whether, proceeding in

the ordinary course of her operations, she suffers an impediment which a physiologist may have produced to thwart her designs. In the first case, she may be provided with an expedient; in the last, she will probably be left without resource.

Here again we may notice the experiment mentioned by NUCK, which, though under similar circumstances, was attended with a different result. Some who feel themselves disposed to venerate his authority, will probably oppose his experiment to mine, and think it incumbent on me to account satisfactorily for the difference. I can by no means acknowledge such an obligation; for to confer validity on experiment by reasoning, is to invert the order of inquiry, and support facts by conjectures. It is sufficient for my credit to be able to adduce evidence of the truth of what I advance, and for this evidence I rely on my preparations.

The train of reasoning which I have lately pursued, led me to extend my inquiries into this particular question still further; and as in the last experiments the vesicles were known to be just on the point of bursting before the tube was cut through; the next step in the inquiry appeared to be, to determine the consequences of dividing the tube a short time after the rudiments of the fœtus had passed. Will the procreative operations be suspended, if the tube be cut through after the ovum is deposited in the uterus?

EXPERIMENT.

I repeated the operation on two rabbits, one of which had received the male two days and eighteen hours, the other two days and twelve hours. I knew from my own experiments, as well as those of DE GRAAF, that the vesicles had

discharged their contents before either of these periods. The examination of these at the usual time, proved that the actions of these parts suffer no interruption by a division of the tube made after the rudiments of the fœtus have been conveyed into the uterus; for there were corporea lutea in both ovaries, and fœtuses in both cornua uteri.

These experiments I think overturn (as far as experiment can) every argument which has hitherto been adduced to support the hypothesis, that the affusion of the semen on the ovaries, either in a sensible form or in that of aura seminalis is essential to impregnation: for if the ovaries were susceptible of their proper excitement only by the contact of semen, by what accident has it happened that the effects of that excitement are not more obvious and further advanced in those experiments, where nothing was done to intercept its course for forty-eight hours, than in those where all communication between the uterus and ovary had been cut off before the means for impregnation had been employed? We should expect in the one case to find the full effects of impregnation, and in the other no traces of it would be seen; instead of which, the procreative actions are no further advanced where there has been an opportunity for the passage of the semen, than in those cases where the passage has been impossible. But if we defer the mutilation until the ovary has perfected its work, which it does in a rabbit in something more than fifty hours from the approach of the male, then the generative process is not disturbed, and the evolution of the fœtus goes on in the usual manner; for now all the different parts of the uterine system being in a condition to act, each performs its peculiar office.

First. The semen by its presence stimulates either the vagina, os uteri, cavity of the uterus, or all of them.

Secondly. The impression made on these is propagated to the ovaries by consent of parts.

Thirdly. One or more of the ovarian vesicles enlarges, projects, bursts, and discharges its contents.

Fourthly. During this process in the ovary, the tube is undergoing a state of preparation for the purpose of embracing the ovary, and receiving the rudiments of the fœtus.

Fifthly. This preparation consists in part of an increased turgescence of its vessels, and a consequent enlargement of its fimbriated extremity. When thus prepared, it approaches the ovary.

Sixthly. After the tube has performed its office by a peristaltic motion, commencing at the fimbriæ, and terminating in the uterus, it gradually returns to its former situation and condition.

Seventhly. While these different actions are going on in the appendages of the uterus, others not less important to the design of nature are instituted in the uterus itself: for the tunica decidua, where it is obvious, is formed ready to secure firmness of connexion between the tender ovum and internal surface of the uterus, until a proper attachment by means of placenta can be effected.

Eighthly. By way of guarding with additional security against a premature escape of the ovum, an apparatus, seated in the neck and mouth of the womb, now begins to develope its real structure, and perform its proper action, consisting in the secretion of a mucus-like substance, sufficient in quantity to fill completely the whole length of the neck, and by that

means to seal up the communication between the cavity of the uterus and vagina.

Ninthly. Nor does the care of nature for the preservation of the new animal terminate here; for while she is by various means forming and perfecting her work, at least as far as comes within the province of the uterine system, she is at the same time making preparation for its nourishment after birth, by instituting the proper secretion of the breasts.

When we take a reflected survey of these successive operations, I think it must appear, on tracing nature's steps through the different stages of this work, *that they are the product of that law in the constitution which is called* SYMPATHY, *or consent of parts.*

That the semen first stimulates the vagina, os uteri, cavity of the uterus, or all of them.

By *sympathy* the ovarian vesicles enlarge, project, and burst.

By *sympathy* the tubes incline to the ovaries, and having embraced them, convey the rudiments of the fœtus into the uterus.

By *sympathy* the uterus makes the necessary preparation for perfecting the formation and growth of the fœtus. And,

By *sympathy* the breasts furnish milk for its support after birth.

Having now investigated this intricate question, I hope with some regularity; the design of this essay next leads me to consider the state or form of that substance which passes from the ovaries in consequence of impregnation.

SECTION III.

What is the Form of that Substance which passes from the Ovaries in consequence of Impregnation?

No sooner had the researches of the physiologists retraced the existence of the new-born animal to the ovaries, than their curiosity was excited to discover the form it assumed while resident in these bodies, and especially at that particular time when the fœtal primordia are about to escape from them. The analogous phænomena of oviparous animals, and the structure of the ovaries as described by DE GRAAF, concurred to favour an opinion, that in viviparous animals there existed ova in these bodies, and indeed from this very circumstance they received their name. But though several physiologists have concurred in this opinion, there has not been any strict coincidence respecting their state while in the ovary. Some have thought that the vesicles described by DE GRAAF were the true ova, and that these are the bodies that are expelled by impregnation. Others, with greater probability, have considered these vesicles as the apparatus destined by nature, under the influence of the proper stimulus, to form the ovum: and though at all times they contain a glairy kind of fluid, from the stimulus of impregnation this fluid becomes a small vesicle or ovum seated within the larger vesicle, which now becoming thickened, and acquiring a yellow colour, is called the corpus luteum: from this body the interior vesicle or ovum is protruded.

Others again refuse assent to both these opinions, and contend that the substance extruded from the corpora lutea has

no vesicular appearance; and though by some it has been called an ovum, yet that name is not applicable to it from any resemblance of figure, but rather from its agreement with an egg in being the substance in which the rudiments of the future animal are contained,

De Graaf contended that the primordia fœtus while in the ovary is vesicular, as appears in his work; in which, after describing the enlargement of the proper vesicles usually connected with his name, he says, "præterea aliquot post coitum "diebus tenuiori substantia præditi sunt, et in sui medio "limpidum liquorem membranâ inclusum continent, quo unà "cum membranâ foras propulso, exigua solum in iis capacitas "superest." He is therefore decidedly of opinion, that as soon as the product of conception becomes the subject of notice, it has a vesicular form, and this he thinks takes place at the end of the third day, though the substance passes from the ovaries several hours before this time. He seems rather to assert, that it passes in a vesicular form, than to prove it; for in fifty-two hours after the approach of the male, he found the ovarian vesicles were empty, though he could not now find the new vesicles either in the uterus or the tubes. But in seventy-two hours they were so evident, that he could distinguish with ease the two membranes of which they are formed, viz. the chorion and amnios; so that they cannot be very small at this time. Hence it would follow, that if on a repetition of this experiment on the third day no vesicles should happen to be found, it would not be from minuteness that they would escape observation; therefore should any one be disposed to search for them, he need not bend his sight, as if looking at microscopical objects.

VALISNERI on the contrary searched for these eggs with great industry, accompanied with an ardent wish to find them; but though his experiments appear to have been judiciously conducted, he never succeeded.

HALLER also maintains, from a regular series of experiments made on sheep (whose term of utero-gestation is five months), that some days elapse between the escape of the substance from the ovaries, and the appearance of a circumscribed body in utero, which can properly be called ovum: and that this does not happen until seventeen days from impregnation. In the mean time, nothing but irregular masses of mucus are found. The circumscribed form at this time acquired seems to depend on the formation of the fœtal membranes now bounding the contained mucus-like substance. This apparently homogeneous mass, on the nineteenth day undergoes a change of character; an opaque spot is seen within it, which subsequent observations prove to be the first evident marks of the evolution or formation of the fœtus. From this dim speck of animal existence we may observe a series of regular advances, from an inorganized mucus-like mass to the most beautiful and complicated machine in nature. But to trace her progressive steps through this important work, forms no part of the design of this dissertation.

The chief difference between DE GRAAF and HALLER on this subject, consists in their opinions respecting the form of the substance that is passing from the ovaries, whether it is vesicular at this time or not; for in the subsequent processes they differ but little. No solution can be given of this question by force of reasoning; it is from experiment alone that we can receive conviction, notwithstanding the two contrary

opinions that prevail. All that can be expected from an individual in such a case, is to add the result of his own labours to one side or the other, so that in the end the preponderance must depend on the weight of evidence.

The experiments I have made on this simple question do not allow me to incline to the side of DE GRAAF; for in the rabbit I have never found any thing in the uterus which had a regular circumscribed form earlier than the sixth day, and even then the substance was bounded by a covering so very tender, that it scarcely had firmness sufficient to support the figure. Before the sixth day, I have never seen any thing but irregular mucus-like masses in the uterus; but after this time the substance has firmness sufficient to admit of preservation in spirits, a specimen of which I have in my collection of preparations.

This acquisition of figure does not depend so much on a difference of consistence, as on the formation of membranes inclosing this substance. These membranes when in a more advanced state of formation, are known by the names of *chorion* and *amnios*. The product of conception being arrived at this stage, may with some propriety be called an ovum, as it has acquired a determined figure; but the different constituent parts of it are not apparent at this early period; on the tenth day, in the rabbit, an opaque spot is seen in this ovum, which increasing daily in its bulk, progressively manifests the formation of the fœtus.

It is a little remarkable that in the rabbit, where the term of utero-gestation does not exceed thirty days, a third part of that time should be required to make that opaque spot obvious to the sight, whilst the remaining two-thirds should suffice to complete the formation of the fœtus. It appears as if it

required a more elaborate exertion of the formative powers of these parts to produce what might figuratively be called the nucleus of a fœtus, than to go on and complete the work. But this remark applies only to the rabbit; for in the human female, abortions at the third month clearly prove that the evolution of the fœtus has been perfected some time before. Such an obvious difference cannot fail to impress our minds with doubts and distrust, whenever we are drawing inferences from analogical reasonings: but to trace the formative process of nature through this work, and to compare her progressive advances in the different periods of utero-gestation, are foreign to the design of this essay.

It remains then for me to beg pardon for having so long trespassed on the patience of this Society.

IX. *Experiments in which, on the third Day after Impregnation, the Ova of Rabbits were found in the fallopian Tubes; and on the fourth Day after Impregnation in the Uterus itself; with the first Appearances of the Fœtus.* By William Cruikshank, *Esq.* *Communicated by* Everard Home, *Esq. F. R. S.*

Read March 23, 1797.

THE ancients imagined that the woman had her testicles, as well as the man, and her own semen. They taught, that in the coitus there was a mixture of the male and female semen in the uterus, and that from a process like fermentation between those two fluids, an embryo was produced. LEWENHOECK said the embryo belonged to the male; and saw, or thought he saw, animalcules in the male semen, resembling the animals to which they belonged. SPALLANZANI says, that the semen of male animals having no animalcules, impregnates as certainly as that of those which have them. This shows that those animalcules are not embryos. STENO, observing that there were round vesicles in the testicles of women, like the eggs of birds, called them ovaria, and said their structure was exactly similar to the ovaria of birds. After this the immortal HARVEY broached the doctrine of "*omnia ab ovo;*" that all animals were produced from ova. "Nos autem asserimus, "animalia omnia, et hominem ipsum, ex quibusdam ovis nasci."

The ova in the ovaria of rabbits are particularly described by DE GRAAF, whence HALLER calls them ova Graffiana.

But the ovaria of quadrupeds often contain vesicles of the hydatid kind; and it becomes difficult to distinguish between what are vesicles, and what are ova. The mark with me is this: the ova are inclosed in a capsule highly vascular from arteries and veins, carrying red blood. The hydatid vesicles are not vascular; at least their vessels carry no red blood. The calyx and the ovum, after impregnation, and even before it, in the state in which the quadruped is said to be *hot*, become black as ink, from the greater derivation of blood; and the ova resemble dark spots: they also come nearer the surface of the ovarium, so as to pout or project, at last, like the nipple in a woman's breast. Some hours after impregnation, the calyx and the coverings of the ovaria burst, and the ovum escapes; may fall into the general cavity of the abdomen, and form an extra-uterine fœtus; but almost always falls into the mouth of the fallopian tube, whose fimbriæ, like fingers, grasp the ovarium, exactly at the place where the ovum is to escape. What the appearance of the ovum was, when deprived of its calyx, or when descending the fallopian tube, was not known. DE GRAAF discovered this in the fallopian tubes of rabbits, in the year 1672; and says, "minutissima "ova invenimus, quæ licet perexigua, gemina, tamen, tunica, "amiciuntur;" and then adds, "hæc quamvis incredibilia, "nobis demonstratu facillima sunt."

DE GRAAF had the fate of Cassandra, to be disbelieved even when he spoke the truth! Dr. HUNTER had his doubts; and the great HALLER, of whom I have always spoke in the language of Professor MARRHAR, "cujus auctoritas apud me plus "valet, quam auctoritas omnium aliorum anatomicorum simul "sumptorum," positively denies their truth. His words are,

"vix liceat admittere"—and afterwards, "denique, quod caput "rei est, neque HARTMANNUS cum experimenta GRAFFIANA ite- "ravit; neque VALISNERUS tot et tam variis in bestiis; neque "ego in pene centum experimentis; neque nuperiorum ana- "tomicorum quispiam, vesiculam, quales sunt in ovariis, post "conceptionem, aut in tuba vidimus aut in utero!"

In the beginning of summer 1778, I was conversing with Dr. HUNTER on this subject, and said, "I should like to repeat "those experiments, now that lectures are over, and that I "have the summer to myself." "You shall make the experi- "ments," said he, "and I shall be at all the expence." Accordingly he carried me to Chelsea, introduced me to a man who kept a rabbit warren, and desired him to let me have as many rabbits as I pleased. I made the experiments; and shall now lay a copy of my journal, then made, before this Society.

EXPERIMENT I.

May 30, 1778. I took a female rabbit, hot, (as the feeders term it) that is, ready to be impregnated, and disposed to receive the male. This they find out, not by exposing her to the male, but by turning up the tail, and inverting part of the vagina: its orifice and internal surface are then as black as ink, from the great derivation of blood to these parts. Having run the point of a double-edged dissecting knife through the spinal marrow, between the atlas and dentata, she instantly expired. I preferred this method of killing her, because when the circulation stopped, the internal parts would be found, respecting vascularity, exactly as in the living body. Upon examination some time after, I found the internal parts of

generation, exactly in the same state as the external; that is, as black as ink: the ovaria had, immediately under their external surfaces, a great number of black, round, bloody spots, somewhat less than mustard seeds. These black spots are the calyces or cups which secrete the ova; they are extremely vascular; the ova themselves are transparent, and carry no visible blood vessels. These calyces, on the expulsion of the ova, enlarge and become yellow, projecting above the external surface of the ovaria, and form the *corpora lutea;* a certain mark of conception in all quadrupeds, and in women themselves, whether the embryo is visible or not. The use of the *corpora lutea* is not yet made out; but the orifice, through which the ovum bursts into the fallopian tube is often extremely manifest, and always has a ragged border, as lacerated parts usually have. The fallopian tubes, independent of their black colour, were twisted like wreathing worms, the peristaltic motion still remaining very vivid; the fimbriæ were also black, and embraced the ovaria (like fingers laying hold of an object) so closely, and so firmly, as to require some force, and even slight laceration, to disengage them.

EXPERIMENT II.

I opened a female rabbit two hours after she received the male: the black bloody spots (just mentioned) now projected much above the surfaces of the ovaria, some of the ruptured orifices were just visible; but in many of these spots there was not the least vestige of an orifice; whence I conclude that they heal very quickly in general. While the animal was yet warm, I injected the arterial system with size coloured with vermilion, whence every thing I had before seen became now

more distinct, and the black spots, which I before conjectured to be congeries of vessels, were now proved to be so.

EXPERIMENT III.

I opened another female rabbit the third day after impregnation: that she was impregnated I could have no doubt, for I never knew impregnation fail if the female was hot, and the male had not been previously exhausted; besides the corpora lutea in the ovaria fully proved it: the appearances were the same as in the last, only the *corpora lutea* were larger; but though I examined the fallopian tubes in the sunshine, and with great care, I could not find any ova, neither in them nor in the horns of the uterus.

EXPERIMENT IV.

I opened another female rabbit the fifth day after conception: the appearances were much the same as in the former animal, only the *corpora lutea* were increased in bulk, but there was not the least vestige of an ovum any where that I could discover. I was now ready to exclaim with HALLER, "vix "liceat admittere."

EXPERIMENT V.

I opened another female rabbit on the eighth day after she had admitted the male: the ova were in the cavity of the uterus, and projected through its substance about the size of a large garden pea; when I cut off the most superior part, and cut into the cavities of the ova, the *liquor amnii* escaped in a proportionate quantity; by their adhesions to the internal surface of the uterus they remained extended, not collapsing in

the smallest degree; the fœtus was not visible; but I had often made the chick, in my experiments on the incubated egg, become visible, by dropping on the spot, where I knew it must be, a drop of distilled vinegar; by dropping the vinegar on the bottom of the little cups I had made, by cutting off the tops of the cells, the fœtus instantly became visible.

EXPERIMENT VI.

Opened another, ninth day: fœtus contained within its amnion, floats in another fluid, between chorion and amnion, which are now at a considerable distance; this fluid jellies in proof spirit. Some *corpora lutea* have cavities, others none, nor the least appearance of orifice. The *corpora lutea* keep increasing as the fœtus increases, are of a sand-red colour, and very vascular.

EXPERIMENT VII.

Opened a doe the eleventh day after coitus: ova very little larger than the last, nor the fœtus: there were but two ova, though several *corpora lutea*. Some pellucid hydatids appeared hanging on the outside of the fallopian tubes. Could these be ova which had missed the passage? they were vascular: the heart of the fœtus was full of blood; the umbilical vessels very distinct, but no chord as yet, contrary to DE GRAAF.

EXPERIMENT VIII.

Opened a doe the fourteenth day: seven corpora lutea in one ovarium, and one in the other; only two ova in the horns of the uterus, one in each; that in the horn next one of the ovaria with one *corpus luteum* was blighted, and the fœtus invisible, even with distilled vinegar; in the other it was increased pro-

portionable to the time; the umbilical chord now for the first time distinct, and the tail detached from the under surface of the uterus; there was something unintelligible about the head, it was bifid on the side next the mouth, with a hole in each extremity; the intestines were now apparent, at least the rectum, as were the lower extremities.

EXPERIMENT IX.

Opened a doe sixth day complete: found the ova loose in the uterus, as described by DE GRAAF, and corresponding nearly to the *corpora lutea*, six in one horn and four in the other; the ova were transparent and of different sizes; they were double, and contained each an internal vesicle, there was a spot on one side in most of them, which I conceived to be the intended point of adherence between them and the uterus; the internal vesicle was not equally in proportion to the external, but in some larger, in others less; I even suspect I saw something of the fœtus: a polypous excrescence in the uterus near the orifice of the fallopian tube, had detained four of the ova at that place; others were scattered in the uterus: just where one of these vesicles had become stationary a white vascular belt was beginning to form, and in the middle of this a cavity where the vesicle lay; the inner membrane I take to be amnion, the outer chorion.

EXPERIMENT X.

Opened a doe the seventh day: the ovaria were shrunk; there were something like three *corpora lutea*, but not distinct; there were two polypi or solid excrescences in the left horn of the uterus, but no ova.

EXPERIMENT XI.

The day after a doe had received the male I made a small opening on the left side of the abdomen, got down upon the uterus just where the fallopian tube goes off, tied the left tube close to the uterus, with a view to intercept the ova. The result of this mentioned afterwards.

EXPERIMENT XII.

Opened a doe the seventh day after coitus: ova all fixed and adhering to the uterus, even making a sensible swell in form of belts at different parts; the amnion appeared in some nearer the chorion than in others; the liquor between amnion and chorion very gelatinous, in many others less so. Saw nothing of fœtus.

EXPERIMENT XIII.

Opened a doe eighth day after coitus: there were about ten or eleven ova; fœtus distinct in almost every one, but not without the application of distilled vinegar for two or three minutes, and afterwards immersed in proof spirit; in some I found the brain, spinal marrow, and vertebræ, forming two columns at some distance; they afterwards gradually approached; for it was in one of the least forward that this was most evident.

EXPERIMENT XIV.

Opened a doe twenty-first day after the coitus: five vessels were seen going out of the navel in one of the fœtuses, besides the urachus; the omphalo-mesenteric artery was very distinct,

and divided into two as it came to the mesentery; could not see the urachus or allantois well, nor the membrane to which the omphalo-mesenteric artery goes.

EXPERIMENT XV.

Opened a doe the fifth day after coitus: found the ova loose in the uterus, to the number of six; even these had a lesser coat in the inside, corresponding to amnion. None in the tubes.

EXPERIMENT XVI.

Opened (fourteen days after the operation) the doe whose fallopian tube I tied. The uterus of the right side was the size of the sixth day; the ovarium and uterus had gone backwards as to the process, and there was no appearance of fœtus; though placenta was very evident on the left side, there was no appearance of conception in the uterus; no placenta; the fallopian tube was very large, soft, and tender; the ovarium twice the size of that on the other side, red, and covered with extravasated coagulable lymph; there was an hydatid in the course of the tube, containing a clear fluid, but nothing like fœtus. I suspect that tying the tube prevented the ova on this side from coming out of the ovarium, and that though they rather increased in the ovarium, the process soon stopped; that the process went on, however, in the other side for a few days, and then stopped likewise: there was universal inflammation about the uterus and colon of the left side, with great quantities of white extravasated coagulable lymph; there was water in the abdomen, and all the appearance of peritoneal inflammation. This process seems to give but little pain, for the animal at the time she was killed was eating and looking as usual.

EXPERIMENT XVII.

Opened a doe the third day after the coitus: the pouting parts of the *corpora lutea* very transparent before the uterus was touched; but as soon as the spermatic and hypogastric arteries were divided, in order to cut out the uterus, they all, as if struck with some shock like electricity, became opaque. The pouting part I believe is the ovum, and stands upon the top of *corpus luteum*; it is very vascular, particularly at its basis, but as soon as perfect, or ready for expulsion, carries no red blood; it continues to grow of itself in utero, without adhering to the uterus for two or three days, then takes root, and becomes very vascular: nothing in the tubes or uterus.

EXPERIMENT XVIII.

Opened another the fourth day in the morning; but it had not conceived, and was in the state of one hot.

EXPERIMENT XIX.

Opened one in the evening of the fourth day: the appearances were little different from those of the fifth morning; the ova were only less dispersed through the uterus, and all accumulated about the orifice of the tubes; the amnion was likewise closer to the chorion.

EXPERIMENT XX.

Opened another at the end of the third day, or rather on the beginning of the fourth: the ovaria were dark brown; the fallopian tubes and uterus almost black, from the great quantity of blood derived to them at this time; I opened this uterus

on the upper edge and in the body, so that the parts all remained turgid; the spermatics and hypogastrics not cut through: the corpora lutea were very vascular, an artery running across ramified from both sides, but particularly spent itself in the centre; the upper part of the corpus luteum, or centre, was a little concave, like the head of a turned small-pock, but no evident foramen: I believe the ova were gone out, but I could see nothing of them in the tubes nor uterus; the fimbriæ were more vascular than I ever saw them, and wholly covered the ovaria; the peristaltic motion of the tubes was very evident, and greater than ever I had seen it; the inner surface of the horns was graniform, with white spots; this I suppose decidua, or perhaps corpus glandulosum EVERRAHDI. DE GRAAF saw the ova in the tubes this day.

EXPERIMENT XXI.

Opened a rabbit at six days and a half: ova in the horns of the uterus were just begun to fix, but did not adhere by vessels; they were very much enlarged compared with the sixth, and the side next the uterus had a round rough spot in it, now very conspicuous; the chorion and amnion were almost in contact with one another; they were easily turned out of the uterus, which embraced them every where loosely, but at the bottom; the *corpora lutea* now increased exceedingly in vascularity, and nourished by a large vessel running across the tubes; remarkably pale, as having done their duty; the graniform appearance on the uterus internally not observable as in the last.

EXPERIMENT XXII.

Opened a doe the seventh day complete after the coitus: turned out, but with difficulty, one of the ova a little larger than in the last; the substance of the uterus over these ova was become thin and transparent, so at first sight you would imagine it was the ovum naked, neither was this part so vascular as one might have expected, considering the principal change was going on here; the ovum burst the moment it was disengaged from the uterus; a gelatinous coagulable fluid issued out, but no appearance of fœtus even in the microscope.

EXPERIMENT XXIII.

Opened another rabbit at the end of the third day: same appearance as in Exp. xx.: searched in vain for the ova on the right side; at last, by drawing a probe gently over the fallopian tube on the left side, before it was opened, more than an inch on the side next the uterus, I pressed out several ova, which seemed to come from about its middle, as I began the pressure there, and the ova did not appear till the very last; the amnion made a centre spot, and appeared small compared to the chorion; no ova in the uterus.

EXPERIMENT XXIV.

Opened another at three days and a half: ovaria had the appearance as if the ova had not yet gone out; however, many of them were found in the uterus, and many in the tubes; I got about six; others were lost, from the great difficulty in slitting up the fallopian tubes without bruising the ova with the fingers or with the point of scissars; there

were eight or nine *corpora lutea* in one ovarium, and two only in the other; on the side of the two I only found one ovum, but twice as large as those on the other side. I observed that the redness of the uterus, depended on not losing much of the animal's blood; for when they had been so killed that much blood was lost, the fallopian tubes at least and ovaria were always pale.

EXPERIMENT XXV.

Opened another rabbit at two days and a half after the coitus: ovaria impregnated, but found no ova in the tubes, nor orifices in the *corpora lutea*.

EXPERIMENT XXVI.

Opened one, third day complete: found about six or seven ova in the fallopian tubes, near their end, or about an inch within the tube, on the side next the uterus: in the microscope the ovum appeared as having three coats; the middle one perhaps becomes allantois or membrana quarta.

EXPERIMENT XXVII.

Opened again another at two days and half: and though there were a great many *corpora lutea*, I could not discover any ova; they were probably too small to be perceived, for on the third day complete some of the ova were not perceptible, till they were put into a fluid, and viewed in the microscope.

EXPERIMENT XXVIII.

Opened one the third day all but two hours: found six ova in one fallopian tube, and seven in the other, which corres-

ponded exactly to the number of *corpora lutea* in each ovarium; the ova had three membranes as before. The circles in the cicatricula of the hen's egg are perhaps similar to these. The ova seem to enlarge in their way down the tube, as a pea swells in the ground before it begins to take root; even in the uterus, for two days, they are either loose and unconnected by vessels, or the vessels are so small as not to be discovered by the microscope. The *corpora lutea* were flatter on the head than I had ever seen them before.

EXPERIMENT XXIX.

I opened another at eight days and a half: every thing more distinct and more advanced than on the eighth day; the heart now visible, and resembling much the appearance of the incubated egg in the forty-eighth hour. There were seven *corpora lutea* in the right ovarium, and but four ova in the right horn of the uterus; there were also three in the left ovarium, though but two ova in the left horn.

GENERAL CONCLUSIONS.

1st. The ovum is formed in, and comes out of the ovarium after conception.

2dly. It passes down the fallopian tube, and is some days in coming through it.

3dly. It is sometimes detained in the fallopian tube, and prevented from getting into the uterus.

4thly. DE GRAAF saw one ovum only in the fallopian tube, "in oviductus dextri medio *unum!*" I saw thirteen in one instance, five in another, seven in another, and three in another, in all twenty-eight.

5thly. The ovum comes into the uterus on the fourth day.

6thly. De Graaf did not see the fœtus till the tenth day; I saw it on the eighth.

7thly. These experiments explain what is seen in the human female. For,

A. I shew a child, at lectures, which remained in the ovaria till it was the size of the fifth month; its fluids were all wasted, and its solids were hard and compressed into an oval form; it had the chorion and amnion, its chord and placenta.

B. I also have in my possession the uterus and ovaria of a young woman who died with the menses upon her; the external membranes of the ovaria are burst at one place, from whence I suspect an ovum escaped, descended through the tube to the uterus, and was washed off by the menstrual blood.

C. The ovum sometimes misses the fallopian tube, falls into the abdomen, and forms the extra-uterine fœtus; this sometimes grows to its full size, labour pains come on at the ninth month, the child may then be taken out alive by the Cæsarean section; or, dying and wasting, but not putrefying, may remain without much inconveniency to the mother for many years.

D. The ovum, although it has gone some way down the fallopian tube, may be arrested in its course and become stationary, and form what is called the fallopian tube case. A remarkable case of this kind is given by Dr. Hunter, in his book on the gravid uterus, where the tube burst, and the mother bled to death.

E. Lastly; the ovum comes into the uterus, where there is room for its enlargement, and a passage for its exit from the body.

P. S. These experiments have been read, and the preparations and engravings shewn, in the lectures on the gravid uterus, given at Windmill-street, every year since the original date of this journal.

EXPLANATION OF THE PLATE (Tab. IV.)

It was not thought necessary to delineate the whole uterus of the rabbit, as it exactly resembles the uterus of other quadrupeds, consisting of a vagina, common to two horns, two fallopian tubes, and two ovaries. Any one who wishes to see this, may see it in DE GRAAF's little book, tolerably well executed for the age in which he lived: but I am more concerned in his first appearances of the ova, than in his general anatomy of the uterus of the rabbit; and therefore proceed to explain the copy of a plate previously engraved, nineteen years ago.

The figures marked 3d day, are ova of the fallopian tube, found after impregnation on that day. The three first are of the natural size; the three next are magnified, in the simple microscope. In all of them the chorion and amnion are even now distinct, and in some of them the *allantois*, as I suspect.

The figures marked $3\frac{1}{2}$ day, are ova still more advanced; similar to which I found many in the tubes, many in the horns of the uterus. The three first are of the natural size; the two following are magnified also in the simple microscope.

The figures marked 4th day, are more enlarged ova in the horns of the uterus, loose, not adhering, capable of being

moved from one place to another (after these horns are opened) by the gentlest breath blown through a blow-pipe.

The figures marked 5th day, are ova of the fifth day; still loose in utero, and still capable of being blown with the gentlest breath from one part to another: they resemble the last in every thing, only that they are larger. The three first are of the natural size; the three last magnified, as the former ova.

The figures marked 6th day, are ova found in the horns of the uterus on that day; sensibly larger than the preceding; not adhering, even now, to the internal surface of the uterus, but exactly as the last in this respect. The four first are of the natural size, the three last magnified as before; but, as kept some years, the amnion has receded from the chorion to a considerable degree.

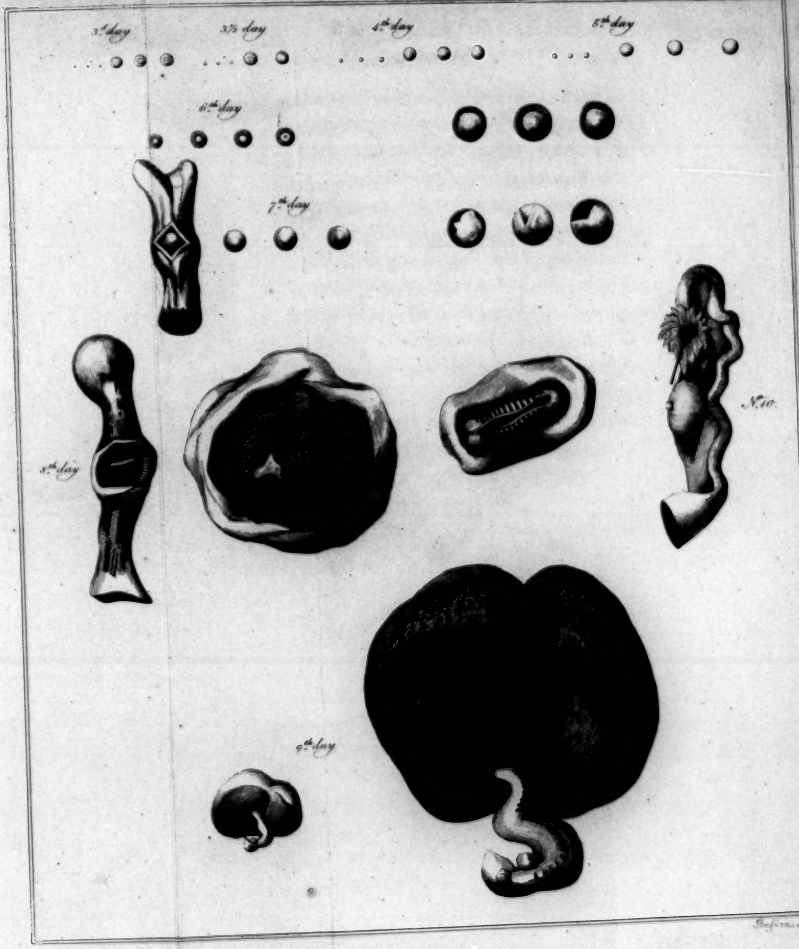
The figures marked 7th day, are ova of the seventh day: the first shews the ovum in its cell in the horn of the uterus, laid open; the three next are similar ova, taken out of their cells, and resembling the former; the three last are of the same period, and also removed from the uterus, but magnified by the same microscope as the preceding ova. They are seen after having been kept many years, and the secession of the amnion from the chorion is still more apparent and greater.

The figures marked 8th day: the first shows the fœtus now first visible to the naked eye by dropping distilled vinegar on it, in one of the cells of the uterus opened. A little above is seen a cell turgid and unopened; and below a cell half divided. The two next figures, in the same line with the fœtus mentioned, are fœtuses of the same period from other rabbits,

magnified. They show the rudiments of the *vertebræ*, and the first appearance of the *spinal marrow*. The third in the same row is also magnified, it shows also the earlier appearances of the two hemispheres of the brain.

Of the figures marked 9th day, one shows the fœtus, now, for the first time, of itself visible to the naked eye, adhering near the tail to the placenta in the closest manner; the navel string as yet too short to be visible, as contrary to DE GRAAF as possible. The second shows the same fœtus magnified.

The figure, on the outside of which is No. 10, shows a *fallopian tube*, on one side of the uterus of the rabbit, with its fimbriated orifice opening into abdomen; and its uterine orifice opening into uterus; also the ovarium, and *corpus luteum* in it, projecting above the surface.


3d day
3½ day
4th day
5th day
6th day
7th day
8th day
9th day
No. 10.

X. *Letter from Sir* Benjamin Thompson, *Knt. Count of* Rumford, *F. R. S. to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S. announcing a Donation to the Royal Society, for the Purpose of instituting a Prize Medal.*

At the Anniversary of the Royal Society, held the 30th of November, 1796, the President acquainted the Society, that Count Rumford had transferred one thousand pounds three *per cent.* consolidated Bank Annuities to the use of the Society, on certain conditions stated in a letter to the President; which was read as follows:

" SIR,

" Desirous of contributing efficaciously to the advancement " of a branch of science which has long employed my atten- " tion, and which appears to me to be of the highest importance " to mankind, and wishing at the same time to leave a lasting " testimony of my respect for the Royal Society of London, I " take the liberty to request that the Royal Society would do " me the honour to accept of one thousand pounds stock, in " the three *per cent.* consolidated public funds of this country; " which stock I have actually purchased, and which I beg leave " to transfer to the President, Council, and Fellows of the Royal " Society; to the end that the interest of the same may be by " them, and by their successors, received from time to time " for ever, and the amount of the same applied and given, once " every second year, as a premium to the author of the most

" important discovery, or useful improvement, which shall be " made and published by printing, or in any way made known " to the public, in any part of Europe, during the preceding " two years, on Heat, or on Light; the preference always being " given to such discoveries as shall, in the opinion of the Pre- " sident and Council of the Royal Society, tend most to pro- " mote the good of mankind.

" With regard to the formalities to be observed by the Pre- " sident and Council of the Royal Society, in their decisions " upon the comparative merits of those discoveries, which in " the opinion of the President and Council may entitle their " authors to be considered as competitors for this biennial pre- " mium, the President and Council of the Royal Society will " be pleased to adopt such regulations as they in their wisdom " may judge to be proper and necessary. But in regard to the " form in which this premium is conferred, I take the liberty " to request, that it may always be given in two medals, struck " in the same die, the one of gold, and the other of silver; and " of such dimensions, that both of them together may be just " equal in intrinsic value to the amount of the interest of the " aforesaid one thousand pounds stock during two years; that " is to say, that they may together be of the value of sixty " pounds sterling.

" The President and Council of the Royal Society will be " pleased to order such device or inscription to be engraved on " the die they shall cause to be prepared for striking these me- " dals, as they may judge proper.

" If, during any term of years, reckoning from the last ad- " judication, or from the last period for the adjudication of this " premium, by the President and Council of the Royal Society,

" no new discovery or improvement should be made in any part " of Europe, relative to either of the subjects in question (Heat or " Light), which, in the opinion of the President and Council of " the Royal Society, shall be of sufficient importance to deserve " this premium; in that case, it is my desire that the premium " may not be given, but that the value of it may be reserved, " and being laid out in the purchase of additional stock in the " English funds, may be employed to augment the capital of " this premium; and that the interest of the same by which " the capital may, from time to time, be so augmented, may " regularly be given in money with the two medals, and as an " addition to the original premium at each such succeeding ad- " judication of it. And it is further my particular request, that " those additions to the value of the premium, arising from its " occasional non-adjudications, may be suffered to increase with- " out limitation.

" With the highest respect for the Royal Society of London, " and the most earnest wishes for their success in their labours " for the good of mankind,

" I have the honour to be, &c.

(signed) " RUMFORD."

London, 12th of July, 1796.

To Sir JOSEPH BANKS, Bart. K. B. President of the Royal Society of London.

The Society hereupon resolved, that they accept of the donation, and accede to the conditions annexed to it by the Count;

and also directed that a letter be written to the Count, acquainting him of this acceptance; returning him thanks for the liberal donation, and assuring him that the conditions annexed to it will be strictly adhered to.

ERRATA.

Page 158, 3d line from the bottom, *for* fig. 8. *read* fig. 10.

Page 205, l. 14, *for* "there was no appearance of conception in the uterus; no placenta;" *read* "there was no other appearance of conception in the uterus; no other placenta;" &c.

METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

OF THE

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL

for January, 1796.

1796	Sir't Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°						
Jan. 1	37	8	0	38	54	30,08	80	0,110	SW	2	Fair.
	44	2	0	44	57	30,03	74		SW	1	Fine.
2	38	8	0	39	51	29,95	81		SW	1	Fine.
	48	2	0	47	57	29,72	76		SSW	1	Cloudy.
3	39	8	0	42	53,5	29,82	80	0,109	SW	1	Cloudy.
	48	2	0	44	57	29,83	82		E	1	Rain.
4	42	8	0	46	54	30,15	86		SW	1	Cloudy.
	49	2	0	48	57	30,19	80		WSW	1	Cloudy.
5	46	8	0	46	55	30,20	81		SSW	1	Cloudy.
	48	2	0	46	57	30,13	80		SSW	1	Cloudy.
6	45	8	0	48	55	30,05	83		S	2	Cloudy.
	51	2	0	46	57,5	30,10	81		NW	1	Cloudy.
7	36	8	0	42	54	30,18	82	0,202	SSW	1	Cloudy.
	50	2	0	49	58	30,14	85		SSW	1	Cloudy.
8	45	8	0	46	55	29,98	82		ESE	1	Cloudy.
	49	2	0	46	59	29,88	87		ESE	1	Cloudy.
9	39	8	0	42	56	29,65	84		ESE	1	Fair.
	50	2	0	46	58,5	29,55	80		ESE	1	Fine.
10	39	8	0	41	56	29,46	82		E	1	Cloudy.
	48,5	2	0	48	57	29,45	82		ENE	1	Cloudy.
11	41,5	8	0	45	55	29,51	85		S	2	Cloudy.
	48	2	0	46	58	29,50	84		SSE	2	Rain.
12	43	8	0	47	55	29,85	84	0,071	S	2	Cloudy.
	53	2	0	52,5	58	29,84	85		S	2	Cloudy.
13	51	8	0	51	57,5	30,00	85		S	2	Cloudy.
	55	2	0	55	55,5	29,95	84		S	2	Cloudy.
14	50	8	0	50	57	29,96	83	0,047	SW	2	Cloudy.
	54	2	0	54	60,5	29,94	77		SSW	2	Hazy.
15	48	8	0	48	59	30,00	75		SW	2	Fine.
	55	2	0	53,5	61	30,11	75		SW	2	Fine.
16	49	8	0	51	59	30,23	81		S	2	Cloudy.
	55	2	0	54	62	30,28	78		S b. W	1	Cloudy.

METEOROLOGICAL JOURNAL

for January, 1796.

1796	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
	°			°	°	Inches.		Inches.			
Jan. 17	45	8	0	46	58	30,32	83		S	1	Fine.
	52	2	0	50	61,5	30,30	81		SSE	1	Fine.
18	42	8	0	46	58	30,14	81		S	1	Fair.
	53	2	0	51	60	30,11	83		S	1	Cloudy.
19	48	8	0	49	58,5	30,08	81		S	2	Fair.
	54	2	0	53	60	29,96	75		S	2	Fair.
20	46	8	0	48	57	29,72	78		S	2	Fair.
	53	2	0	52	60	29,78	75		SSW	2	Cloudy.
21	50	8	0	54	58	29,61	77		SSW	2	Cloudy.
	56	2	0	55	60,5	29,68	72		SE	1	Fair.
22	47	8	0	47	58	29,68	74		SE	1	Fine.
	56	2	0	53	62	29,58	69		S	2	Cloudy.
23	49	8	0	49	59	29,59	76		S	2	Cloudy.
	52,5	2	0	51,5	60	29,40	73		SSW	2	Fine. [much wind last night.
24	47	8	0	47	57	29,26	74	0,049	W	2	Fair.
	50,5	2	0	46	58	29,49	73		S	3	Cloudy. [much wind last night.
25	45	8	0	48	55	29,15	78	0,101	S	2	Rain.
	50,5	2	0	48	58	29,15	77		S	2	Cloudy.
26	42,5	8	0	44	56	29,30	77	0,070	S	2	Rain.
	50	2	0	49	57	29,12	80		SSW	2	Cloudy.
27	42	8	0	42	55	29,24	76	0,422	SSW	2	Fair.
	48,5	2	0	48,5	58	29,35	73		S	1	Rain.
28	42	8	0	42	56	29,24	80	0,202	SSW	1	Rain.
	47	2	0	45	58	29,18	80		S	2	Cloudy.
29	40	8	0	47	56	29,02	80	0,375	S	2	Cloudy.
	50	2	0	49	58	29,00	75		SSE	1	Fair.
30	41	8	0	42	54	29,03	80	0,155	S	2	Cloudy.
	50	2	0	49,5	58	29,04	76		SSE	1	Cloudy.
31	44	8	0	44	54	29,09	81	0,215	SE	1	Cloudy.
	48	2	0	48	57	29,05	80				

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		H. M.			Inches.		Inches.	Points.	Str.	
	o		o	o						
Feb. 1	43	7 0	43	54	29,06	80	0,118	S	2	Rain.
	48	2 0	44	56	29,09	78		S	2	Rain.
2	38	7 0	39	54	29,28	79	0,038	ESE	2	Fair.
	45,5	2 0	44	57	29,27	77		SE	2	Cloudy.
3	33	7 0	33,5	53,5	29,65	81	0,130	SW	1	Fair.
	44	2 0	42	57	29,61	76		S b. E	2	Fair.
4	36	7 0	37,5	54	29,55	81		N	1	Cloudy.
	45	2 0	45	56,5	29,68	75		NNW	1	Fair.
5	36,5	7 0	40	54	29,18	82	0,172	S b. E	2	Rain.
	48	2 0	46	56	29,20	73		SSW	2	Cloudy.
6	39	7 0	39	54	29,15	80		SW	1	Fair.
	46	2 0	46	57	29,27	76		WNW	1	Cloudy.
7	35	7 0	36	54	29,45	80	0,064	SW	1	Cloudy.
	45	2 0	44	55	29,28	76		SE	1	Cloudy.
8	40	7 0	42	53	29,18	80	0,170	S	1	Fair.
	49	2 0	48,5	55,5	29,09	78		S	1	Cloudy.
9	37,5	7 0	38,5	54	29,05	83	0,121	SW	1	Cloudy.
	46,5	2 0	46	56	29,18	77		N	1	Cloudy.
10	39	7 0	39	53	29,70	78	0,020	NE	2	Cloudy.
	41	2 0	41	53	29,92	74		NE	2	Cloudy.
11	32	7 0	33	52	30,23	77		N	1	Fair.
	42	2 0	41	54	30,22	75		SSW	1	Cloudy.
12	40	7 0	44	53	29,93	85	0,102	S	2	Rain.
	52	2 0	52	55	29,70	86		S	2	Cloudy.
13	35	7 0	35	53	29,64	78	0,208	SW	1	Fine.
	44	2 0	40	55	29,54	77		NNE	2	Cloudy.
14	36	7 0	37	53	29,57	78		WNW	1	Cloudy.
	47	2 0	46,5	55	29,57	71		NW	2	Fair.
15	35	7 0	35,5	52,5	29,81	75		NNW	2	Fine.
	44,5	2 0	44,5	56	29,88	74		N	1	Fair.
16	35	7 0	36	53	30,04	75		W	1	Fair.
	48	2 0	48	55	30,05	73		SW	1	Cloudy.

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	°			°	°	Inches.		Inches.			
Feb. 17	42	7	0	43	53	29,97	80		SW	1	Fair.
	50	2	0	50	56	30,00	70		W	1	Cloudy.
18	39,5	7	0	40	54	30,07	80		SSW	1	Cloudy.
	48,5	2	0	48	56	30,05	74		SSW	1	Cloudy.
19	48	7	0	48,5	55,5	30,00	81		WSW	1	Cloudy.
	56	2	0	55,5	58	30,06	76		WNW	1	Cloudy.
20	46	7	0	46	58	30,10	78		WNW	1	Cloudy.
	51,5	2	0	51	58,5	30,10	72		WNW	1	Cloudy.
21	43	7	0	43	57	30,15	76		WSW	1	Cloudy.
	47	2	0	47	58,5	30,15	74		W	1	Cloudy.
22	43	7	0	43	56	30,07	73		E	1	Cloudy.
	44	2	0	44	58	30,05	73		ESE	1	Cloudy.
23	35	7	0	39	56	30,05	79		ENE	1	Cloudy.
	48,5	2	0	47,5	58	30,04	71		ENE	1	Fair.
24	34	7	0	35	55	30,13	78		ENE	1	Fair.
	45	2	0	44,5	57,5	30,16	66		ENE	1	Fine.
25	38	7	0	35	55	30,30	79		E	1	Cloudy.
	43	2	0	42	56	30,31	74		NE	1	Cloudy.
26	38	7	0	38	55	30,30	75		NE	1	Cloudy.
	43	2	0	43	57	30,31	74		NE	1	Cloudy.
27	37	7	0	38	54	30,31	78		NE	1	Cloudy.
	42,5	2	0	41	56,5	30,24	67		NE	2	Cloudy.
28	30	7	0	30,5	52	30,28	71		NE	2	Cloudy.
	37	2	0	36	53,5	30,25	71		NE	1	Cloudy.
29	30	7	0	31	51	30,09	77		E	1	Snow.
	33	2	0	32,5	54	30,14	68		NE	2	Fair.

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1796	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.		Inches.			
Mar. 1	27	7	0	30	49	29,88	71		NE	2	Cloudy.
	39	2	0	38	51	29,88	68		NE	2	Cloudy.
2	33	7	0	35	49	29,82	73		NE	2	Cloudy.
	41	2	0	40	51	29,82	67		NE	2	Fair.
3	33	7	0	33	48,5	29,80	70		NE	1	Cloudy.
	37,5	2	0	36	52	29,71	65		E	1	Fair.
4	28,5	7	0	31,5	48,5	29,87	71		NE	1	Cloudy.
	36,5	2	0	36	51	29,95	65		NE	1	Cloudy.
5	26,5	7	0	27	48	30,13	70		E	1	Cloudy.
	38	2	0	37	52	30,19	58		ESE	1	Fine.
6	29	7	0	30	49	30,32	64		SE	1	Cloudy.
	33	2	0	30,5	50	30,35	71		E	1	Cloudy.
7	29	7	0	29	47	30,30	68		NE	1	Cloudy.
	32	2	0	31,5	50	30,29	68		NE	2	Cloudy.
8	27,5	7	0	29	48	30,28	69		ENE	1	Cloudy.
	38	2	0	37,5	50	30,23	68		ENE	1	Fair.
9	33	7	0	34	48	30,06	74		NE	1	Cloudy.
	43	2	0	42	50	30,00	71		E	1	Fair.
10	30	7	0	32	48,5	29,97	80		NE	1	Fair.
	48	2	0	48	53	29,95	67		E	1	Fair.
11	33	7	0	34	50	29,91	79		E	1	Fair.
	45	2	0	45	54	29,90	78		E	1	Fair.
12	40	7	0	42	52	29,98	79		E	1	Cloudy.
	53	2	0	50	55	30,03	76		SSE	2	Cloudy.
13	46	7	0	46	54	30,17	84		SSW	1	Cloudy.
	52	2	0	52	56	30,21	79		S	1	Cloudy.
14	42	7	0	44	54	30,26	83		SSE	1	Fair.
	54	2	0	54	57	30,23	65		S	1	Fair.
15	43	7	0	46	54	30,18	81		S	1	Hazy.
	59	2	0	58	58	30,18	68		SSE	2	Fine.
16	42	7	0	43	56	30,17	76		ENE	1	Fair.
	60	2	0	59	58,5	30,18	66		E	1	Hazy.

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		H. M.			Inches.		Inches.	Points.	Str.	
	°									
Mar. 17	39	7 0	40	56	30,19	75		ENE	1	Fair.
	56	2 0	56	59	30,18	62		E	1	Fine.
18	37	7 0	38	58	30,12	72		ENE	1	Fine.
	55	2 0	55	60	30,10	63		E	1	Fine.
19	37	7 0	38	58	30,26	74		ENE	1	Fine.
	54	2 0	53	59	30,32	63		E	1	Fine.
20	35	7 0	39	58	30,44	74		NE	1	Cloudy.
	46	2 0	46	59	30,42	66		NE	1	Cloudy.
21	39	7 0	42	55	30,36	72		NE	1	Cloudy.
	51	2 0	51	60	30,36	65		NE	1	Fine.
22	41	7 0	42	58	30,35	70		NE	1	Cloudy.
	49	2 0	49	59	30,30	67		NE	1	Cloudy.
23	41	7 0	42	55	30,22	72		NE	1	Cloudy.
	47	2 0	47	57	30,19	67		W	1	Cloudy.
24	40	7 0	41	55	29,99	70		W	1	Cloudy.
	48	2 0	48	56	29,90	68		N	2	Fine.
25	34	7 0	35	54	29,90	68		NE	1	Fine.
	44	2 0	44	56	29,94	59		NW	1	Cloudy.
26	41	7 0	46	55	29,73	74		NW	1	Cloudy.
	54,5	2 0	53	58	29,72	68		NE	2	Snow.
27	33	7 0	33	56	29,50	77		NE	2	Fine.
	39	2 0	38	57	29,63	66		NNE	2	Cloudy.
28	30	7 0	32	54	29,68	75		NE	2	Cloudy.
	42	2 0	42	56	29,70	68		W	1	Fine.
29	28	7 0	31	53	29,80	72	0,043	W	1	Cloudy.
	47	2 0	45	54	29,79	69		WSW	1	Cloudy.
30	36	7 0	40	54	29,66	74		S	1	Cloudy.
	51	2 0	48	56	29,56	76		SW	1	Cloudy.
31	42	7 0	42	55	29,74	79	0,031	SW	1	Cloudy.
	55	2 0	54	57	29,81	70				

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1796	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.		Inches.			
Apr. 1	45	7	0	47	56	29,83	82		S	1	Cloudy.
	56	2	0	56	57	29,88	79		SSE	1	Cloudy.
2	46	7	0	46	57	29,93	81		E	1	Cloudy.
	59	2	0	58	59	29,96	73		ESE	1	Cloudy.
3	49,5	7	0	51	58	30,10	82	0,068	E	1	Rain.
	58	2	0	57,5	60	30,11	78		E	1	Cloudy.
4	43	7	0	45	58	30,16	81	0,212	E	1	Hazy.
	63	2	0	61	61	30,14	76		E	1	Fair.
5	42	7	0	45	59	30,15	75		E	1	Fine.
	56	2	0	56	61	30,15	67		E	1	Fine.
6	37	7	0	40	58	30,17	77		E	1	Fine.
	51	2	0	49	61	30,16	63		E	1	Fine.
7	37	7	0	40	58	30,20	78		E	1	Hazy.
	52	2	0	50,5	59	30,19	62		E	1	Cloudy.
8	36	7	0	41	58	30,26	77		ENE	1	Hazy.
	51,5	2	0	49	58	30,22	66		ENE	1	Cloudy.
9	38	7	0	40	57	30,11	75		NE	1	Fair.
	50	2	0	48,5	58	29,99	64		NE	1	Cloudy.
10	37	7	0	42	56	29,96	77		NE	1	Fine.
	50	2	0	49	57	29,96	68		NE	1	Cloudy.
11	39	7	0	41	55	29,95	78		NE	1	Cloudy.
	50	2	0	49	57	29,92	68		NE	1	Cloudy.
12	41,5	7	0	44	55	29,95	75		NE	1	Cloudy.
	50,5	2	0	49	57	29,98	68		NE	1	Cloudy.
13	36	7	0	39	55	30,11	76		NE	1	Cloudy.
	51	2	0	50,5	57	30,11	66		NE	1	Cloudy.
14	39	7	0	42	55	30,13	71		W	1	Cloudy.
	55,5	2	0	55	58	30,10	63		NW	1	Cloudy.
15	45	7	0	48	57	30,20	72		NW	1	Cloudy.
	63	2	0	62	59,5	30,20	66		NW	1	Fair.
16	50	7	0	50	58	30,18	76		W	1	Cloudy.
	61	2	0	58	60	30,14	68		W	1	Cloudy.

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		H. M.	°	°	Inches.		Inches.	Points.	Str.	
	°									°
Apr. 17	47,5	7 0	50	59	30,14	73		WNW	1	Cloudy.
	62	2 0	60	61	30,14	68		NW	1	Cloudy.
18	46	7 0	47	59	30,13	77		W	1	Hazy.
	65	2 0	64	62	30,16	66		W	1	Cloudy.
19	45	7 0	48	60	30,16	77		ENE	1	Cloudy.
	64,5	2 0	64	63	30,13	64		ESE	1	Fine.
20	45	7 0	50	61	30,12	70		E	1	Fine.
	60	2 0	60	63	30,06	61		E	2	Fine.
21	45	7 0	50	61	30,00	71		E	2	Fine.
	65	2 0	64	64	29,99	63		E	2	Fine.
22	48	7 0	52	63	30,10	69		E	1	Fair.
	67	2 0	66	64	30,10	59		E	1	Fine.
23	47	7 0	50	63	30,10	72		E	1	Hazy.
	70	2 0	68,5	64,5	30,06	62		E	1	Cloudy.
24	50	7 0	53	63	30,04	76		WSW	1	Cloudy.
	60	2 0	59	64	30,08	63		NW	1	Fair.
25	42	7 0	47	62	30,20	70		NE	2	Fine.
	57	2 0	56	63	30,19	64		NE	2	Fair.
26	43	7 0	46	61	30,32	72		NE	2	Fine.
	60	2 0	58	64	30,30	63		NE	1	Fine.
27	41	7 0	42	61	30,30	74		NE	1	Cloudy.
	56	2 0	55	62	30,22	67		SE	1	Hazy.
28	45	7 0	47	61	29,97	73				Fair.
	65	2 0	65	63,5	29,81	59				Fine.
29	45	7 0	48	62	29,50	72		SW	1	Cloudy.
	61,5	2 0	60	63	29,36	62		SSE	1	Fair.
30	45	7 0	47	61	29,14	74		E	2	Cloudy.
	56,5	2 10	51	62	29,08	72	0,022	SE	2	Rain.

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1796	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°						
May 1	40,5	7	0	44,5	61	29,16	73		SW	1	Fair.
	59	2	0	56	62	29,18	64		ESE	1	Cloudy.
2	42	7	0	48	60	29,35	73		ENE	1	Fair.
	59	2	0	58	62,5	29,43	65		ENE	1	Fair.
3	45	7	0	46	60	29,60	79	0,105	E	1	Rain.
	49	2	0	49	60	29,71	70		E	1	Cloudy.
4	41,5	7	0	44	59	29,81	72	0,102	NE	1	Cloudy.
	50	2	0	50	59	29,82	67		NE	1	Cloudy.
5	39	7	0	44	58	29,84	73		NE	1	Fair.
	53	2	0	52	59	29,84	63		NE	1	Cloudy.
6	39	7	0	45	57	29,90	70		NE	1	Cloudy.
	55	2	0	53	58	29,88	72		NE	1	Cloudy.
7	42	7	0	47	58	29,96	74	0,117	WSW	1	Cloudy.
	60	2	0	58	59	29,91	64		S	1	Fair.
8	46	7	0	52	58	29,53	85	0,370	W	1	Rain.
	58	2	0	57	60	29,69	71		WNW	1	Cloudy.
9	50	7	0	53	59	29,65	75	0,236	SW	2	Fine.
	64	2	0	63	62	29,69	64		WSW	2	Cloudy.
10	50	7	0	52	60	29,66	75	0,264	SW	2	Fair.
	63	2	0	62	62	29,66	65		SW	2	Fair.
11	50	7	0	52	61	29,55	77	0,116	SSW	2	Fair.
	62	2	0	60	62	29,62	66		SSW	2	Fair.
12	49	7	0	52	60	29,72	75	0,120	SSW	2	Fair.
	62	2	0	59	63	29,69	68		SW	2	Fair.
13	45	7	0	47	60	29,41	74	0,103	SSW	2	Fair.
	59	2	0	57	61	29,52	67		SSW	2	Fair.
14	45	7	0	47	60	29,86	74	0,172	NW	1	Cloudy.
	57,5	2	0	57	61	29,91	65		NW	1	Fair.
15	41	7	0	44	60	29,87	77	0,154	NW	1	Fine.
	57	2	0	55	61	29,82	70		NW	1	Cloudy.
16	41	7	0	44	59	29,85	76	0,056	NNW	1	Fair.
	55	2	0	53	60,5	29,95	71		ENE	1	Fair.

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	°			°	°						
May 17	39	7	0	44	59	30,22	73	0,021	ENE	1	Fine.
	57	2	0	56	60	30,22	64		E	1	Fine.
18	43	7	0	50	58,5	30,12	78		E	1	Fine.
	62,5	2	0	62	61,5	30,04	68		E	2	Fine.
19	49,5	7	0	53	60	29,98	72		E	2	Fine.
	63,5	2	0	62	61	29,94	61		E	2	Fine.
20	47,5	7	0	52	60	29,85	72		ENE	2	Fine.
	65	2	0	63,5	62	29,84	61		E	2	Fine.
21	49	7	0	53	61	29,72	82		NE	1	Cloudy.
	57	2	0	54	61,5	29,71	78		ENE	1	Cloudy.
22	48	7	0	52	61	29,82	74		NW	1	Cloudy.
	63	2	0	61	62	29,85	66		W	1	Fair.
23	48	7	0	50	60	30,00	80	0,040	NW	1	Rain.
	62,5	2	0	61	62	30,03	69		NW	1	Cloudy.
24	48	7	0	49	61	30,10	77		NE	1	Cloudy.
	57,5	2	0	57	62	30,02	71		NE	1	Cloudy.
25	45	7	0	46	60	29,84	80		ENE	1	Cloudy.
	64	2	0	63	63	29,76	66		ENE	1	Fine.
26	47	7	0	51	61	29,86	71		WSW	1	Fair.
	69	2	0	67	62	29,79	64		WSW	1	Cloudy.
27	50	7	0	56	61,5	29,78	74		W	1	Cloudy.
	65	2	0	64	63	29,74	67		W	1	Fair.
28	46	7	0	50	61,5	29,86	75		W	1	Hazy.
	62	2	0	59	62	29,80	65		SW	2	Cloudy.
29	47	7	0	50	60	29,55	73	0,058	SSW	2	Fine.
	61	2	0	57	61	29,53	70		SSW	2	Cloudy.
30	49	7	0	51	59	29,00	80	0,164	S	2	Rain.
	58	2	0	57	60	28,94	68		S	2	Cloudy.
31	48	7	0	50	58	29,18	80	0,305	SW	1	Rain.
	60	2	0	59	60	29,34	70		SW	2	Fair.

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1796	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	o		o	o						
June 1	47	7 0	51	60	29,72	72	0,072	SSW	2	Fair.
	62	2 0	60	60	29,72	67		SSW	2	Fair.
2	45	7 0	50	59	29,69	70		SSE	2	Fair.
	60	2 0	57	59	29,56	67		SE	2	Hazy.
3	49	7 0	51	59	29,44	79	0,156	NNE	1	Cloudy.
	58	2 0	58	59	29,50	70		NW	1	Cloudy.
4	47	7 0	48	59	29,62	77	0,076	WNW	1	Cloudy.
	58	2 0	58	59	29,78	69		NW	1	Fair.
5	46	7 0	51	59	30,04	74		WSW	1	Hazy.
	66	2 0	65	60	30,05	63		S	1	Hazy.
6	49	7 0	51,5	59	30,09	79	0,058	NNE	1	Cloudy.
	64	2 0	62	60	30,12	67		NE	1	Cloudy.
7	52	7 0	53	60	30,13	77		SW	1	Fair.
	72	2 0	70	61	30,07	65		WSW	1	Cloudy.
8	57	7 0	58	61,5	29,96	80		SSW	1	Cloudy.
	68,5	2 0	67	63	29,90	65		WSW	1	Cloudy.
9	53	7 0	54,5	61,5	29,80	72		WSW	1	Cloudy.
	68	2 0	67	62	29,87	62		NW	1	Cloudy.
10	48	7 0	51	62	30,10	74		E	1	Fair.
	68	2 0	67	63	30,00	66		S	2	Fair.
11	50	7 0	55	62	29,81	76		SW	1	Hazy.
	73	2 0	72	63	29,77	67		SSW	1	Hazy.
12	51	7 0	57	63	29,77	73		WNW	1	Cloudy.
	62	2 0	61	63,5	29,80	73		SW	2	Cloudy.
13	56	7 0	58	63	30,03	73		SW	2	Cloudy.
	63	2 0	63	63	30,04	71		SW b.S	2	Cloudy.
14	53	7 0	55	62	29,98	71		SSW	2	Cloudy.
	63	2 0	63	62,5	29,92	69		SSW	2	Cloudy.
15	45	7 0	49	61	30,04	74	0,035	WSW	2	Fine.
	64	2 0	63	62	30,04	61		WSW	2	Fair.
16	46	7 0	52	61	30,16	73		SW	2	Fair.
	62	2 0	58	61,5	30,08	79		SSW	2	Cloudy.

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1796	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°									
June 17	51	7 0	55	61	30,21	80	0,033	SW	1	Cloudy.
	67	2 0	66	63	30,17	72		SSW	1	Cloudy.
18	58	7 0	59	62	30,22	77	0,056	SW	1	Cloudy.
	70	2 0	69	63	30,24	64		WSW	1	Cloudy.
19	52	7 0	56	62	30,11	67		E	1	Hazy.
	72	2 0	72	63,5	29,91	62		SSE	1	Cloudy.
20	53	7 0	55	63	29,67	73		W	1	Cloudy.
	63	2 0	61	63	29,60	63		WSW	1	Cloudy.
21	46	7 0	53	61	29,77	71		WNW	2	Cloudy.
	64	2 0	63	63	29,83	63		WNW	2	Cloudy.
22	50	7 0	53	61	29,64	83	0,026	S	1	Rain.
	66	2 0	63	62	29,64	71		W	1	Cloudy.
23	53	7 0	56	62	29,87	74	0,024	W	1	Cloudy.
	68	2 0	67	63	29,96	63		NW	1	Fair.
24	54	7 0	58	63	30,16	74		NW	1	Fair.
	73	2 0	72	65	30,22	61		N	1	Fine.
25	54	7 0	58	64	30,25	72		SSW	1	Fine.
	75	2 0	73,5	66	30,19	64		S b. E	1	Fine.
26	54	7 0	59	65	29,96	72		ESE	1	Fine.
	80	2 9	78	68,5	29,83	65		S	2	Fine.
27	56	7 0	57	66	30,02	65		N	2	Cloudy.
	65	2 0	61	66	30,03	63		NE	1	Cloudy.
28	53	7 0	57	65	30,09	67		NE	1	Cloudy.
	65	2 0	61	64	30,09	64		E	1	Cloudy.
29	53	7 0	57	64	30,22	68		E	1	Fair.
	68	2 0	67	65	30,23	60		NE	1	Fair.
30	49	7 0	53	64	30,31	72		E	1	Fine.
	73	2 0	72	66	30,22	59		E	1	Fine.

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1796	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.		Inches.			
July 1	53	7	0	57	64	30,18	69		E	1	Cloudy.
	73	2	0	73	60	30,15	67		E	1	Fine.
2	56	7	0	61	64,5	30,04	74		E	1	Cloudy.
	70,5	2	0	68	66	30,00	69		NW	1	Cloudy.
3	53	7	0	56	65	29,81	73	0,031	NW	1	Fair.
	65	2	0	63	66	29,80	65		NE	1	Cloudy.
4	49	7	0	54	64	29,81	71		N	1	Cloudy.
	66	2	0	65	65	29,80	61		W	1	Cloudy.
5	55,5	7	0	58	64	29,52	81		SSW	2	Cloudy.
	68	2	0	67	65	29,40	65		SW	2	Fair.
6	51	7	0	55	64	29,39	76	0,281	SSW	2	Cloudy.
	65	2	0	60,5	64	29,37	67		SSW	2	Cloudy.
7	48	7	0	52	63	29,78	72	0,075	SW	2	Cloudy.
	65	2	0	64	64	29,83	61		W	2	Fair.
8	47,5	7	0	52	62	29,82	74	0,131	SSW	2	Cloudy.
	61	2	0	56	62	29,75	73		SW	2	Cloudy.
9	44,5	7	0	50	61	29,94	77	0,218	SW	2	Fine.
	64	2	0	61	62	29,94	73		SW	2	Cloudy.
10	54	7	0	56	61	29,65	86	0,350	SW	2	Rain.
	62	2	0	61,5	62	29,71	68		SW	2	Cloudy.
11	49	7	0	52	61	29,83	75		WNW	2	Fine.
	65	2	0	64	62	29,82	64		WNW	2	Cloudy.
12	46,5	7	0	51	60	29,94	76	0,167	WNW	1	Fine.
	68,5	2	0	67,5	62	29,96	61		WNW	2	Fair.
13	55	7	0	57	61	29,74	81		SW	1	Cloudy.
	72	2	0	71	63	29,78	63		SW	1	Hazy.
14	58	7	0	62	62	29,81	82	0,018	SSW	2	Cloudy.
	75	2	0	74,5	64	29,87	66		SW	2	Fair.
15	60	7	0	63	64	29,94	76		S	2	Fair.
	77,5	2	0	76,5	66	29,90	65		SSE	2	Fine.
16	63	7	0	63,5	67	29,62	74		S	2	Fair.
	72	2	0	71	67	29,62	67		S	2	Cloudy.

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1796	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.		Inches.			
July 17	56	7	0	58	66	29,75	73		SSW	2	Fine.
	69	2	0	67	67	29,81	65		SSW	2	Fair.
18	54	7	0	58	63	29,92	77		S	2	Cloudy.
	70	2	0	68	65	29,87	63		SSE	2	Cloudy.
19	56	7	0	56	65	29,65	79	0,173	S	2	Cloudy.
	63	2	0	63	65	29,78	70		SW	2	Cloudy.
20	50	7	0	54	64	30,11	77		WSW	1	Fine.
	70	2	0	68,5	65	30,11	64		SW	1	Cloudy.
21	54	7	0	59	64	29,87	75		SSW	1	Cloudy.
	70	2	0	69	65	29,88	61		SW	2	Fair.
22	53	7	0	55	64	29,85	73		SW	1	Fair.
	67	2	0	66	65	29,81	62		SW	1	Fair.
23	52	7	0	55	64	29,72	74	0,030	SW	1	Fine.
	68	2	0	66	64	29,71	64		WNW	1	Cloudy.
24	53	7	0	56	63	29,54	81	0,186	SW	2	Rain.
	72	2	0	69	65	29,59	65		W	2	Fair.
25	58	7	0	60	64	29,56	80	0,025	W	2	Fair.
	71	2	0	71	66,5	29,56	66		SSW	2	Fair.
26	57	7	0	58	65	29,55	77	0,031	S	2	Fair.
	67	2	0	66	65	29,54	67		S	2	Fair.
27	55	7	0	57	64	29,67	76	0,030	S	1	Fair.
	69	2	0	67	66	29,67	64		SSW	1	Cloudy.
28	53	7	0	56	64	29,84	80	0,080	SSW	2	Fine.
	66	2	0	62	64	29,85	74		S	2	Cloudy.
29	57,5	7	0	58	64	29,77	80	0,078	SSW	2	Cloudy.
	73,5	2	0	72	66	29,84	67		SW	2	Fair.
30	56	7	0	59	65	29,93	75		NE	1	Hazy.
	71	2	0	71	66	29,93	66		E	1	Hazy.
31	56	7	0	58	65	29,93	82		ESE	1	Cloudy.
	74	2	0	73	66	29,91	67		SSE	2	Fair.

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1796	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Scr.	
	°										
Aug. 1	60	7	0	62	66	29,79	83		SSW	1	Cloudy.
	68	2	0	64	66	29,78	81		SSW	1	Rain.
2	60,5	7	0	62	66	29,80	78	0,093	SSW	2	Cloudy.
	68	2	0	66	66	29,80	75		SSW	2	Cloudy.
3	56,5	7	0	56,5	65,5	29,73	82	0,210	W	1	Cloudy.
	66	2	0	64	66	29,86	68		NW	1	Cloudy.
4	48,5	7	0	52	64,5	30,01	77		SW	1	Fine.
	66,5	2	0	65	64	30,02	63		SW	1	Cloudy.
5	51	7	0	53	64	30,14	78		SW	1	Fine.
	69,5	2	0	68	66	30,11	64		SW	2	Fair.
6	54	7	0	58	64	30,06	76		SW	2	Cloudy.
	69	2	0	66	65	30,01	70		SW	2	Cloudy.
7	50	7	0	53	64	30,22	76		WNW	1	Fine.
	72	2	0	71	65	30,19	62		WNW	1	Fine.
8	52	7	0	57	64	30,12	73		SW	1	Fine.
	74	2	0	74	66	30,01	63		S	1	Fine.
9	56	7	0	58	65	29,93	75		SW	1	Cloudy.
	73	2	0	72	67	29,85	68		S	1	Fine.
10	58	7	0	60	66	29,71	79		W	1	Cloudy.
	72	2	0	71	68	29,82	72		WNW	1	Fair.
11	52	7	0	56	66	30,14	76		WSW	1	Hazy.
	71	2	0	71	67	30,14	62		NE	1	Hazy.
12	52	7	0	58	66	30,08	76		E	1	Hazy.
	72	2	0	72	68	30,03	63		E	1	Fine.
13	55,5	7	0	57,5	66	30,15	75		ENE	1	Hazy.
	77	2	0	74	68	30,17	62		NE	1	Fine.
14	57	7	0	62	67	30,20	75		NE	1	Cloudy.
	77	2	0	77	68	30,19	64		E	1	Cloudy.
15	58	7	0	61	68	30,29	74		ENE	1	Fine.
	73	2	0	72	70	30,31	69		E	1	Fine.
16	52	7	0	57	67,5	30,41	78		NE	1	Fair.
	71	2	0	71	69	30,39	62		E	1	Fine.

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1796	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°						
Aug. 17	49	7	0	55	67	30,37	72		NE	1	Cloudy.
	68,5	2	0	68	68	30,32	63		E	1	Fine.
18	56	7	0	59	67	30,22	67		NE	1	Cloudy.
	70	2	0	69	69	30,15	64		E	1	Fine.
19	53	7	0	58	68	30,10	80		NE	1	Cloudy.
	71	2	0	71	69	30,06	65		E	1	Fine.
20	55	7	0	60	68	30,05	83		E	1	Cloudy.
	74	2	0	73	70	30,02	59		E	1	Fine.
21	57	7	0	60	69	30,00	80		E	1	Hazy.
	78	2	0	78	72	30,00	63		E	1	Fair.
22	58	7	0	63	70	30,10	72		E	1	Fair.
	80	2	0	80	72	30,11	59		NE	1	Fine.
23	56,5	7	0	59	70	30,17	80		NE	1	Cloudy.
	74,5	2	0	74	71,5	30,14	67		NE	1	Fine.
24	56	7	0	58	70	30,14	60		ENE	1	Cloudy.
	76	2	0	74	72	30,09	65		E	1	Fine.
25	57	7	0	58	68	30,08	77		NE	1	Cloudy.
	74,5	2	0	73	71	30,03	65		NE	1	Fair.
26	56	7	0	58	68	30,03	79		SW	1	Cloudy.
	73	2	0	73	70	29,98	67		WSW	1	Cloudy.
27	54	7	0	57	69	29,90	77	0,098	W	1	Fair.
	65	2	0	63	69	29,98	64		NW	1	Fair.
28	48	7	0	53	68	30,14	75		SW	1	Fine.
	65	2	0	64	67	30,11	62		WNW	2	Fair.
29	51,5	7	0	55	67	29,98	77		NW	2	Cloudy.
	64	2	0	61	68	29,98	73		NW	2	Cloudy.
30	52,5	7	0	56	65	30,04	78	0,071	NE	2	Cloudy.
	64	2	0	60	66	30,04	80		NNE	1	Cloudy.
31	54	7	0	56	66	29,98	80	0,057	NE	1	Cloudy.
	61	2	0	61	66	29,97	83		N	1	Cloudy.

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1796	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.		Inches.			
Sept. 1	53,5	7	0	54	65	29,97	79	0,038	N	1	Cloudy.
	63	2	0	62	65	29,92	65		N	1	Cloudy.
2	51	7	0	53	64	29,97	77		NE	1	Fine.
	62	2	0	62	64	30,01	65		N	1	Cloudy.
3	50	7	0	53	64	30,08	79		NE	1	Cloudy.
	65,5	2	0	65	64,5	30,03	65		W	1	Fair.
4	53	7	0	56	63	29,88	85		W	1	Fair.
	67	2	0	66	64	29,88	82		WSW	1	Cloudy.
5	53	7	0	56	64	30,02	75	0,169	WSW	1	Fair.
	65	2	0	64	64	30,06	69		W	1	Cloudy.
6	52	7	0	56	63,5	30,05	81		SW	1	Cloudy.
	68	2	0	68	65	29,96	68		SW	1	Fair.
7	59	7	0	61	65	29,79	78		SW	2	Cloudy.
	68,5	2	0	67	65,5	29,82	77		W	2	Cloudy.
8	52,5	7	0	56	65	29,96	82		SW	1	Fine.
	72	2	0	71	65	29,99	66		WNW	1	Cloudy.
9	57	7	0	59	66	30,05	80		SW	1	Fair.
	72,5	2	0	71,5	67	30,06	69		S	1	Cloudy.
10	59	7	0	60	65,5	30,01	78		NE	1	Fair.
	76	2	0	75	67	30,01	67		S	1	Fair.
11	61	7	0	63	67	30,06	81		S	1	Cloudy.
	73	2	0	73	68	30,06	72		SW	1	Fair.
12	53	7	0	55	67	30,21	81		SSW	1	Cloudy.
	71	2	0	70	67,5	30,22	73		S	1	Fair.
13	53	7	0	56	66,5	30,16	81		SW	1	Cloudy.
	74	2	0	73,5	68	30,09	67		S	2	Fine.
14	55	7	0	57	67	30,12	81		SSW	1	Cloudy.
	74,5	2	0	74	68	30,08	69		SW	1	Fine.
15	61	7	0	63	68	30,00	80		SW	2	Fair.
	74	2	0	74	70	30,01	67		SW	2	Fair.
16	61,5	7	0	62	68,5	30,10	80		SSW	2	Fair.
	71,5	2	0	71	71	30,10	68		SSW	2	Fine.

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1796	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.		Inches.			
Sept. 17	56	7	0	58	69	30,04	81		SSW	1	Cloudy.
	79	2	0	78	71	30,01	72		S	1	Fine.
18	58	7	0	61	70	29,94	79		SW	1	Fine.
	75,5	2	0	73	72	29,91	68		WNW	1	Fine.
19	60	7	0	61	70,5	29,83	85	0,380	ENE	1	Cloudy.
	62	2	0	62	70	29,78	83		NE	1	Rain.
20	60	7	0	62	69	29,57	85	0,062	SE	1	Cloudy.
	70	2	0	68	70	29,49	77		ESE	2	Cloudy.
21	59	7	0	59	69	29,46	83	0,316	S	1	Rain.
	68	2	0	67	69	29,57	73		S	1	Fair.
22	52	7	0	55	68	29,72	82	0,122			Foggy.
	64	2	0	63	68	29,73	76		E	1	Fair.
23	52	7	0	52	66,5	29,86	81	0,116	NE	1	Cloudy.
	59	2	0	58	66	29,87	75		NE	1	Cloudy.
24	51	7	0	52	65	29,92	78	0,018	NE	1	Cloudy.
	61	2	0	61	65	29,92	77		NE	1	Cloudy.
25	50	7	0	56	64	29,85	85		NE	1	Cloudy.
	61	2	0	57	64	29,79	84		NE	1	Rain.
26	54,5	7	0	56	64	29,81	88	0,320	NE	1	Cloudy.
	59	2	0	58	64	29,86	86		NE	1	Cloudy.
27	54	7	0	54	63	30,01	85		NE	1	Cloudy.
	61	2	0	61	64	30,05	73		NE	1	Fair.
28	52	7	0	54,5	63	30,08	81		NE	1	Cloudy.
	61	2	0	60	63	30,10	80		NE	1	Cloudy.
29	49	7	0	50	62	30,14	80		NE	1	Fine.
	60	2	0	59	63,5	30,18	69		NE	1	Cloudy.
30	45	7	0	46	61	30,28	78		NE	1	Fine.
	57	2	0	56	62	30,28	66		NE	1	Fair.

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1796	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.		Inches.	Points.	Str.	
	°									
Oct. 1	40	7 0	42	60	30,21	77		WSW	1	Cloudy.
	56	2 0	55	60	30,17	72		NW	1	Cloudy.
2	49,5	7 0	50	60	30,22	77		W	1	Cloudy.
	59	2 0	59	60	30,28	71		WNW	1	Cloudy.
3	50	7 0	53	59,5	30,29	75		W	1	Cloudy.
	58	2 0	58	60	30,32	69		WNW	1	Cloudy.
4	53	7 0	53	59	30,24	73		W	1	Cloudy.
	57	2 0	57	59,5	30,17	70		W	1	Cloudy.
5	53	7 0	53	59	29,82	72		SSW	2	Fair.
	58	2 0	56	59	29,71	75		SSW	2	Cloudy.
6	51	7 0	52	59	29,40	85	0,210	S	1	Cloudy.
	58	2 0	57	59,5	29,44	69		S	2	Cloudy.
7	45	7 0	46	58	29,38	76	0,061	S	2	Fair.
	53	2 0	52	58	29,31	78		SSE	2	Fair.
8	42	7 0	43	57	29,48	81	0,092	SSW	1	Fair.
	55	2 0	54	58	29,50	68		W	1	Fair.
9	48	7 0	50	57	29,38	86	0,220	S	1	Rain.
	57,5	2 0	56	58	29,17	85		WSW	1	Rain.
10	43,5	7 0	45	56	29,65	73	0,095	W	2	Fine.
	54,5	2 0	54	57	29,70	65		WNW	2	Fair.
11	42	7 0	43	56	29,50	80		WNW	2	Fine.
	53	2 0	48	56	29,38	76		WNW	2	Fair.
12	38,5	7 0	40	54,5	29,48	78	0,036	W	1	Fine.
	55	2 0	54	56,5	29,49	69		W	2	Fine.
13	46	7 0	46	55	29,42	80	0,120	S	1	Rain.
	51	2 0	51	57	29,46	74		NW	1	Fair.
14	42	7 0	43	56	29,67	81	0,110	SW	1	Fine.
	55	2 0	55	59	29,64	76		S b. E	1	Cloudy.
15	45	7 0	46,5	57	29,61	85	0,530	W	1	Cloudy.
	49	2 0	48,5	58	29,68	82		NE	1	Cloudy.
16	39	7 0	40	56	29,95	84		NE	1	Fine.
	51	2 0	50,5	59	30,02	74		NE	1	Fair.

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1796	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°						
Oct. 17	38	7 0	39	56	30,18	85		ENE	1	Fair.
	53	2 0	53	59	30,05	76		NE	1	Fair.
18	44	7 0	46	57	29,73	83	0,080	NE	1	Rain.
	50,5	2 0	50	58	29,72	76		NW	1	Cloudy.
19	41	7 0	45	56	29,85	83	0,120	W	1	Cloudy.
	51	2 0	51	58	30,01	73		N	1	Cloudy.
20	46,5	7 0	46,5	57	30,24	79		WNW	1	Cloudy.
	55	2 0	54,5	59	30,26	75		WNW	1	Cloudy.
21	50	7 0	50	58	30,27	84		SW	1	Cloudy.
	54	2 0	54	60	30,25	79		SW	1	Cloudy.
22	50	7 0	50	59	30,21	85		SW	1	Cloudy.
	56	2 0	55	60	30,16	78		SW	1	Cloudy.
23	51	7 0	51	59	30,18	80		WSW	1	Cloudy.
	56	2 0	54	61	30,03	82		W	1	Fair.
24	36,5	7 0	38	58	30,16	77	0,031	NW	1	Fair.
	46	2 0	45	59	30,26	69		NW	1	Fair.
25	30	7 0	32	56	30,50	75		NW	1	Fine.
	44,5	2 0	44	57	30,55	66		NE	1	Fine.
26	35	7 0	36	55	30,55	80		NE	1	Cloudy.
	49,5	2 0	48	57	30,47	80		NE	1	Fine.
27	41	7 0	41	55	30,38	84	0,098	NE	1	Fair.
	50	2 0	49	58	30,36	71		NE	1	Cloudy.
28	46	7 0	46	56	30,21	80		NE	1	Cloudy.
	52	2 0	50	58	30,14	77		E	1	Cloudy.
29	46	7 0	46	57	30,08	82		E	1	Cloudy.
	50	2 0	50	58	30,05	79		E	1	Cloudy.
30	47	7 0	47	57	29,98	83		NE	1	Cloudy.
	49	2 0	49	58	29,98	79		W	1	Cloudy.
31	45	7 0	46	56	30,08	79		NW	1	Cloudy.
	52,5	2 0	52,5	58	30,11	72				

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1796	Six's Therm. least and greatest Heat.	Time. H	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
	°			°	°	Inches.		Inches.			
Nov. 1	45	7	0	47	56	30,19	82		W	1	Cloudy.
	56	2	0	56	58	30,13	80		WSW	1	Cloudy.
2	52	7	0	52	57	30,00	84		SW	1	Cloudy.
	57	2	0	57	59	29,93	80		WSW	1	Cloudy.
3	52	7	0	52	58	29,78	85		WNW	1	Cloudy.
	54	2	0	54	60	29,75	68		WNW	1	Cloudy.
4	42	7	0	42,5	57	29,57	80		W	1	Fine.
	49	2	0	45	56	29,55	76		NW	1	Fine.
5	39	7	0	42	56	29,86	79		NNW	1	Cloudy.
	45	2	0	45	58	30,03	69		NNE	1	Fine.
6	30	7	0	30	55	30,12	76		SW	1	Fine.
	47	2	0	47	57	29,96	74		SW	1	Fair.
7	40	7	0	42	54	29,69	85		SW	1	Cloudy.
	49,5	2	0	49,5	57	29,60	74		S	1	Hazy.
8	37	7	0	38	55	29,50	81		W	1	Cloudy.
	47	2	0	47	57	29,53	77		NE	1	Fair.
9	41	7	0	41	55	29,78	84		NE	1	Cloudy.
	47,5	2	0	47	56	29,99	80		NE	1	Cloudy.
10	42	7	0	43	54	30,00	84		NE	1	Cloudy.
	45	2	0	45	56	29,98	80		ENE	1	Cloudy.
11	37	7	0	37	54	29,96	82		E	1	Cloudy.
	44	2	0	44	55	29,94	79		E	1	Cloudy.
12	39	7	0	39	53	29,84	83		NE	1	Cloudy.
	46	2	0	46	54	29,77	82		NE	1	Cloudy.
13	39	7	0	39	53	29,68	81		NE	1	Cloudy.
	44	2	0	44	53	29,71	73		NE	1	Fair.
14	38	7	0	38	52	29,74	75		E	1	Cloudy.
	40	2	0	40	53	29,70	72		E	1	Cloudy.
15	36	7	0	36	52	29,70	82		W	1	Cloudy.
	41	2	0	41	54	29,84	71		NW	1	Cloudy.
16	33	7	0	34	52	29,91	80		S	1	Cloudy.
	45	2	0	44	52	29,63	82		SSE	2	Rain.

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1796	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°						
Nov.17	40	7	0	43	52	29,18	83	0,192	SSW	1	Cloudy.
	43	2	0	43	53	29,30	83		WNW	1	Cloudy.
18	33,5	7	0	36	52	29,34	79	0,153	NW	1	Cloudy.
	42	2	0	42	53	29,33	82		NW	1	Cloudy.
19	39	7	0	39	52	29,38	84	0,110	NW	1	Cloudy.
	42,5	2	0	42	53	29,35	82		NE	1	Cloudy.
20	36	7	0	36	52	29,46	85		NE	1	Cloudy.
	42	2	0	42	53	29,51	83		NE	1	Cloudy.
21	31	7	0	32	51,5	29,64	84	0,087		1	Foggy.
	42	2	0	35	53	29,66	85		NE	1	Cloudy.
22	42	7	0	42	52	29,65	88	0,387	NE	1	Rain.
	49	2	0	47,5	53,5	29,69	87		SE	1	Cloudy.
23	44	7	0	44	52	29,83	87	0,280	E	1	Cloudy.
	47	2	0	46	55	29,88	86		E	1	Cloudy.
24	42	7	0	43	53	29,98	85		E	1	Cloudy.
	47	2	0	47	55	30,00	80		NE	1	Cloudy.
25	42	7	0	43	54	30,18	80		E	1	Cloudy.
	43	2	0	43	55	30,25	82		E	1	Cloudy.
26	41	7	0	41	53	30,28	79		NE	1	Cloudy.
	43	2	0	43	55	30,28	76		NE	1	Cloudy.
27	42	7	0	42	53	30,27	80		NE	1	Cloudy.
	44,5	2	0	44	55	30,22	85		NE	1	Cloudy.
28	40	7	0	40	53	30,29	81		NE	1	Cloudy.
	42	2	0	42	54	30,27	77		NE	1	Cloudy.
29	34	7	0	35	52	30,27	78		W	1	Cloudy.
	39	2	0	39	54	30,16	75		WNW	1	Fair.
30	29	7	0	29	50	30,00	74		NW	1	Fine.
	30	2	0	30	48	30,00	68		NW	1	Fine.

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1796	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
	°			°	°	Inches.		Inches.			
Dec. 1	24	8	0	24	47	29,98	75		NW	1	Fine.
	32	2	0	32	49	29,99	73		NW	1	Fine.
2	23,5	8	0	21,5	47	30,05	77		W	1	Fine.
	34	2	0	34	48	30,07	76		SW	1	Fine.
3	22	8	0	25	46	29,90	77		W	1	Cloudy.
	35	2	0	34	47	29,77	78		SW	1	Fair.
4	35	8	0	35	47	29,57	84		SW	1	Cloudy.
	36	2	0	32	47	29,69	78		NW	1	Fair.
5	23	8	0	25	44	29,74	81		W	1	Fine.
	36	2	0	30	47	29,62	76		SW	1	Fine.
6	24	8	0	25	44	29,82	80	0,262	NW	1	Fair.
	35	2	0	32	48	29,99	74		NW	1	Fine.
7	23	8	0	24	44	30,13	82		NW	1	Fair.
	37	2	0	35	45	30,03	80		NW	1	Cloudy.
8	26	8	0	28	45	30,00	75		NW	1	Fine.
	37	2	0	37	47,5	30,04	73		NW	1	Fair.
9	27	8	0	26	46	30,33	81		NW	1	Fine.
	35	2	0	33	48	30,34	78		NW	1	Cloudy.
10	23	8	0	24	45	30,51	81		E	1	Cloudy.
	30	2	0	29	48	30,50	78		SW	1	Fine.
11	31	8	0	27	46	30,27	85		SSW	1	Cloudy.
	34	2	0	34	45	30,30	86		WNW	1	Cloudy.
12	33,5	8	0	34	44	30,34	85	0,034	N	1	Rain.
	39	2	0	39	48	30,35	85		NNE	1	Cloudy.
13	35	8	0	35	46	30,30	87		NE	1	Cloudy.
	37	2	0	37	49	30,26	85		NE	1	Cloudy.
14	33,5	8	0	37	47	30,07	83		NE	1	Cloudy.
	41	2	0	41	50	30,07	84		NE	1	Cloudy.
15	35	8	0	35	48	30,14	83		NE	1	Cloudy.
	36	2	0	34	50	30,18	84		NE	1	Cloudy.
16	33	8	0	33	48	30,16	84		ENE	1	Cloudy.
	35	2	0	35	50	30,20	82		E	1	Cloudy.

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1796	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°						
Dec. 17	31	8 0	31	48	30,15	80		E	1	Cloudy.
	32	2 0	32	50	30,05	80		E	1	Cloudy.
18	30	8 0	32	48	29,61	85	0,138	E	1	Rain.
	35	2 0	35	50	29.42	86		E	1	Rain.
19	35	8 0	38	48	29.28	88	0,435			Foggy.
	48	2 0	46,5	53	29.24	89		SSW	1	Cloudy.
20	40	8 0	40	50	29,27	89	0,096	SW	1	Rain.
	42	2 0	39	53	29.41	84		NE	1	Cloudy.
21	28	8 0	28	49	29,61	85		SW	1	Fine.
	33	2 0	33	51	29,65	82		SSW	1	Cloudy.
22	28	8 0	28	49	29,58	80		NE	1	Cloudy.
	31	2 0	31	50	29,61	74		NE	1	Fair.
23	25	8 0	26	48	29.29	87		NE	1	Snow.
	32	2 0	32	50	29.36	83		NE	1	Cloudy.
24	19	8 0	20	47	29,63	80		N	1	Fair.
	23	2 0	23	49	29.68	76		N	1	Fair.
25	4	8 0	5	43	29.73	80				Foggy.
	23	2 0	16	46	29.72	80		NE	1	Fair.
26	16	8 0	23	43	29,62	82		E	2	Fair.
	29	2 0	29	45	29.59	79		E	2	Cloudy.
27	26,5	8 0	26,5	43	29,57	80		E	2	Cloudy.
	29	2 0	29	47	29,63	80		E b. S	2	Cloudy.
28	26,5	8 0	30	43	29,40	85	0,200	E b. S	2	Rain.
	37	2 0	35	47	29,44	88		E	1	Cloudy.
29	35	8 0	36	45	29,47	90	0,068			Foggy.
	45	2 0	45	48	29,47	90		SE	1	Cloudy.
30	44	8 0	46	48	29,54	90	0,041	S b. W	2	Rain.
	51,5	2 0	49	52	29,54	84		S b. W	2	Rain.
31	46	8 0	46	51	29,53	83	0,035	S	2	Cloudy.
	48	2 0	48	54	29,53	86		S	2	Cloudy.

1796.	Six's Therm. without.			Thermometer without.			Thermometer within.			Barometer.*			Hygrometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.
January	56	36	47,3	55	38	47,5	62	51	57,2	30,32	29,00	29,72	86	73	79,3	2,128
February	56	30	41,7	55,5	30,5	41,7	58,5	51	55,0	30,31	29,05	29,81	86	66	76,3	1,143
March	60	26,5	41,0	59	27	41,4	60	47	54,0	30,44	29,50	30,03	84	58	70,7	0,074
April	70	36	50,9	68,5	39	51,4	64,5	55	59,8	30,32	29,08	30,04	82	59	70,4	0,302
May	65	39	52,7	64	44	54,0	63	57	60,4	30,22	28,94	29,73	85	63	71,4	2,301
June	80	45	58,8	78	49	59,8	68,5	59	62,2	30,31	29,44	29,96	83	59	69,7	0,536
July	77,5	44,5	61,2	76,5	50	62,0	67	60	64,1	30,18	29,37	29,79	86	61	71,2	1,904
August	80	48,5	62,5	80	52	63,7	72	64	67,2	30,41	29,71	30,06	83	59	71,5	0,529
September	79,8	45	61,9	78	46	61,4	72	61	66,1	30,28	29,46	29,96	88	65	75,1	1,541
October	59	30	48,7	59	32	48,9	61	54,5	57,8	30,55	29,17	29,94	86	65	77,2	1,803
November	57	29	42,2	57	29	42,2	60	50	54,3	30,29	29,18	29,83	88	68	80,0	1,209
December	51,5	4	32,1	49	5	32,1	53	43	47,5	30,51	29,24	29,83	90	73	81,9	1,309
Whole year			50,1			50,5			58,8			29,89			74,6	14,779

* The quicksilver in the bason of the barometer is 81 feet above the level of low water spring tides at Somerset-house.